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From

The Revd Chas: H. Cotter M. A. Dean:
Sawarden of St. Augustine's
College, Canterbury.
Oct 13th / 92

To

Merton Russell Cotter Esq. F.R.G.S.
of Bournemouth
with a kinsman's greeting.

J. Turner

Roger Cotes succeeded Sir Isaac
Newton in the chair of mathematics
at Cambridge. He wrote the Preface
to Newton's Principia. On his
early demise Sir Isaac said
of him, "If Mr. Cotes had lived
we should have known some-
thing."

C. H. C.

58689

HYDROSTATICAL
AND
PNEUMATICAL
LECTURES

BY

ROGER COTES A.M.

Late Professor of Astronomy and Experimental
Philosophy at Cambridge:

PUBLISHED WITH

N O T E S

By his Successor

ROBERT SMITH LL.D.

Master of Mechanicks to His Majesty.

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HIS ROYAL HIGHNESS

WILLIAM

DUKE OF CAMBRIDGE



LECTURE

AND PRACTICE

OF

HIS ROYAL HIGHNESS

WILLIAM

DUKE OF CAMBRIDGE

TO
HIS ROYAL HIGHNESS
WILLIAM
DUKE OF CUMBERLAND,
THESE
LECTURES

ARE HUMBL Y DEDICATED,

BY

HIS ROYAL HIGHNESS'S

MOST DUTIFUL SERVANT

ROBERT SMITH.

T H E E D I T O R S

F A C E E

The Editor of the *Journal of the Royal Society of Medicine* has been informed by the Author that the paper was not sent to him in time for the issue of the 1st of the month. The Editor has therefore been obliged to postpone the paper to the issue of the 1st of the next month. The Editor has also been informed by the Author that the paper was not sent to him in time for the issue of the 1st of the month. The Editor has therefore been obliged to postpone the paper to the issue of the 1st of the next month.

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T H E E D I T O R ' S

P R E F A C E.

THE Course of Hydrostatical and Pneumatical Experiments for which these Lectures were written, was contrived by the Author above thirty years ago, and was one of the first that we had in England of any considerable note. From that time it has been often performed before large assemblies at the Observatory in Trinity College Cambridge; first by the Author in conjunction with Mr. *Whiston*, at that time Professor of the Mathematicks, then by the Author alone, and after his decease by my self: and on these occasions the Lectures were frequently lent out to be transcribed; by which means a copy falling lately into the hands of a Bookseller, was intended to have been printed without my knowledge. By this attempt I was induced to present the Publick with a correct edition of them, printed from the Author's original manuscript.

The general heads of the Course and of the Lectures upon it, may be seen at one view in the following paper, in which every article marked with a Star, referring to a page in the Book, is the subject of a Lecture to be found by that reference. And the whole design is farther explained by the Author in the beginning of his first and twelfth Lectures, where he gives his reasons why he judged it needless to write up-

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on the subject of any more articles than those we have here distinguished.

As to the method in general of teaching Philosophy by Courses of Experiments, (that is, of drawing general truths and conclusions from a select number of simple experiments, first represented to our senses, and then explained to our understandings,) it is now so much practised and approved of by the most eminent Professors all over Europe, and has so greatly contributed to the propagation and increase of knowledge, in the little time it has been duly cultivated, that nothing more need be said to shew the usefulness and excellency of it: and as to the Lectures before us, the general satisfaction they have given, to all those curious Persons who have perused them in manuscript, or heard them read, together with the great and established reputation of their Author, will recommend them to the Publick, much beyond any thing I can offer in their commendation.

In comparing some other Works of our Author (*a*) with the Lectures before us, the reader will find this remarkable difference in the style and manner of writing. In those other works he is generally sparing of his words and thoughts, that he might not seem tedious to able Mathematicians; in these he is more liberal and diffusive in thought and expression, and condescends to clear up every appearance of difficul-

(*a*) *Harmonia Mensurarum*, sive analysis & synthesis per rationum & angulorum mensuras promotæ, &c.

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ty, as writing either for beginners or for persons of common understandings; and he has rendered these Discourses more entertaining and useful than ordinary, by enlivening the Science with a mixture of Learning, and the History of the Inventions he treats of; in which he has been particularly careful to do justice, and to give the deserved honour to their several Inventors.

Two or three noble Truths relating to the Pressure of fluids (*b*), and the constitution of the Atmosphere (*c*), the Author has here demonstrated, from the very first mathematical Elements, with such uncommon plainness and perspicuity, as cannot fail of giving pleasure to all sorts of mathematical Readers. As to the rest, I am much mistaken if Readers of good sense and some command of attention, though unskilled in Mathematicks, may not go through it with as much ease and pleasure, as in reading a piece of history; and upon making the tryal, they may possibly conceive a better opinion of their own understandings, with regard to these matters, than ever they did before.

Nevertheless it may probably be surmised, as these Discourses were written on purpose to deduce the properties of fluids from a Course of Experiments, actually performed and explained before the reading of each Lecture, that therefore they may not be so well adapted to Readers in

(*b*) Lect. iii. (*c*) Lect. ix. and xiv.

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ral, as to those who have seen that Course, or some other of the same kind. It must indeed be confessed, that the latter Readers would have some advantage over the former, in as much as the Author intending to shew the experiments, belonging to each Lecture, immediately before the reading of it, does not always describe them, but frequently reasons upon them as things already seen and understood. But as I have taken due care to supply such defects, by giving compleat descriptions of the Experiments in Notes, referring to very good Figures of the proper *Apparatus*, I presume I have entirely removed the suspicion abovemention'd; and have rendered the lectures as clearly intelligible, and as well adapted to the taste and capacity of all sorts of readers in the form they now have, as if the matter of the Notes had been every where inserted in the context of the Lectures, even by the Author himself. I have also added here and there a few Mathematical Notes, upon such points as the Author had but lightly touched upon, or only mentioned, but thought improper to be introduced into popular discourses.

When he exhibited and explained his Instruments for determining the state of the weather, as Barometers, Thermometers and Hygrometers of various sorts, for a Lecture on this subject he constantly read Dr. *Halley's Account of the rising and falling of the mercury in the barometer, upon change of weather*, as what he perfectly approved. With the Doctor's leave I have therefore

P R E F A C E.

therefore reprinted that excellent Discourse of his from the Philosophical Transactions, and placed it in an Appendix to these Lectures: and have added to it a Translation of Sir *Isaac Newton's Scale of degrees of heat*, taken also from the Transactions; as directing us, among other great curiosities, to such a Construction of Thermometers, as shall cause them to denote the same degree of heat, though they have never been actually compared together: which useful property is still wanting in all the Thermometers I have yet seen. I have therefore fully described this Construction at the end of that admirable paper.

For the uses of Hygrometers Mr. *Cotes* referred his pupils chiefly to Mr. *Boyle's* Tracts upon that subject.

Lastly, because in the Lecture upon Capillary Tubes many particulars are wanting, which since the Author's decease have been discovered and explained by Dr. *Jurin*, his particular friend, as well as my own, I could not do better, with the Doctor's leave, than close my Appendix with his very ingenious Discourses upon that curious subject: and to make the Book more useful, I have added an alphabetical Index of the matters contained in it.

THE HEADS OF THE

The heads of a Course of Hydrostatical and Pneumatical Experiments, as performed at the Observatory in Trinity-college, CAMBRIDGE.

Hydrostatical tryals and conclusions.

THAT fluids gravitate in proprio loco, the upper parts continually pressing upon the lower; that this pressure is not only propagated downwards, but even upwards and side-ways, according to all possible directions; that a lighter fluid may gravitate upon an heavier, and an heavier upon a lighter. * Page 1. Lect. i.

That a fluid may sustain a body heavier in specie than itself, and even raise it up; that a fluid may detain a body lighter in specie than itself, and even depress it; that a competent pressure of a fluid may produce the remarkable phænomena of the Torricellian tube, pump, syringe, siphon, polished plates, and other effects of the like nature. * Page 10. Lect. ii.

That fluids press according to their perpendicular altitudes, whatever be their quantities, or however the containing vessels be figured; the exact estimate of all manner of pressures; the invention of the center of pressure, upon any proposed plain, reduced to the problem of finding

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Lect. iii.*

*Of the sinking and floating of bodies immersed in fluids, their relative gravities and levities, their situations and positions: the phænomena of glass bubbles accounted for. * Page 37.
Lect. iv.*

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Lect. v.*

*The praxis of the hydrostatical ballance; the specifick gravities of several particular bodies actually found out; with an account of the various uses of such enquiries. * Page 62.
Lect. vi.*

Pneumatics illustrated by experiments for the most part tubular, being such as were wont to be made before the air-pump was invented.

*THE several phænomena of the Torricellian experiment exhibited and explained. * Page 71.
Lect. vii.*

*Monfieur Pascal's imitation of the same experiment by water; other experiments of the like nature with fluids variously combined; the pressure of the air, shewn by experiment to be different at different altitudes from the surface of the earth. * Page 82. Lect viii.*

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*The density and spring of the air proved to be as the force which compresses it, and from hence an enquiry is made into the limits and state of the atmosphere. * Page 94. Lect. ix.*

*The effects of the weight and spring of the air in syringes, pumps, siphons, polished plates, cupping glasses, suction, respiration, &c. * Page 106. Lect. x.*

Instruments for determining the state of the weather.

*The phænomena of capillary tubes, glass planes, the figures of the surfaces of fluids, and other things relating to the same head, considered. * Page 115. Lect. xi.*

The more known properties of the air established by the air-pump, and other engines.

*THE air-pump, the instruments for condensing and transferring air, their fabrick, operation, and gages explained. * Page 126. Lect. xii.*

*An account of the several successive degrees in which the air is expanded and compressed by the air-pump and Condenser. * Page 137. Lect. xiii.*

A parcel of air weighed in the ballance; its specific gravity to that of water determined thereby:

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*thereby: a second enquiry into the state of the atmosphere. * Page 153. Lect. xiv.*

The weight, pressure and spring of the air proved several ways; by the sense of feeling, by breaking glass vials, by the phænomena of bladders, glass bubbles, fountains, the gardener's watering pot, the diving bell, &c.

Syphons, syringes, polished plates, the Torricellian tube in vacuo; quicksilver raised to the usual height of the weather-glass by the bare spring of a little included air; Otto Guericke's hemispheres.

The ebullition of liquors in vacuo, the quantity of air contained in them, the sustentation of fumes and vapours, the descent of bodies in vacuo, the refraction of air.

The more hidden properties of the air considered by the help of the like engines.

The influence of the air examined as to the causes of magnetism, the elasticity of springs, the sphericity of the drops of fluids, the ascent of liquors in capillary tubes, the reflection of light from the farther surface of glasses, &c.

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GEORGE R.

GEORGE the Second, by the Grace of God, King of *Great-Britain, France and Ireland*, Defender of the Faith, &c. To all, to whom these Presents shall come, Greeting. Whereas Our Trusty and wellbeloved **ROBERT SMITH**, Doctor of Laws, Professor of Astronomy and Experimental Philosophy, in Our University of *Cambridge*, has humbly represented unto Us, that he has, at his own Expence, Printed and prepared for Publication, a Book in Octavo, Intituled *Hydrostatical and Pneumatical Lectures* by Roger Cotes, *A. M. late Professor of Astronomy and Experimental Philosophy at Cambridge, published with Notes by his Successor Robert Smith, LL. D.* to the Copy of which he has the Sole Right and Title; and has therefore humbly besought Us, to grant him Our Royal Privilege and Licence for the Sole printing and publishing the said Book, for the Term of Fourteen Years; We, being willing to give all due Encouragement to Works of this Nature, that tend to the Advancement of Learning, are graciously pleased to condescend to his Request; and do therefore, by these Presents, so far as may be agreeable to the Statute in that behalf made and provided, grant unto him, the said **ROBERT SMITH**, his Heirs, Executors and Assigns Our Royal Privilege and Licence for the sole printing and publishing the said Book, for the Term of Fourteen Years, to be computed from the Date hereof; strictly forbidding and prohibiting all Our Subjects within Our Kingdoms and Dominions to reprint, abridge or translate the same, either in the like or any other Volume or Volumes whatsoever; or to import, buy, vend, utter or distribute any Copies of the same, reprinted beyond the Seas, during the said Term of Fourteen Years, without the consent and approbation of the said **ROBERT SMITH**, his Heirs, Executors, and Assigns, by Writing under his, or their Hands and Seals first had and obtained, as they, and every of them offending herein, shall answer the contrary at their Perils; Whereof the Commissioners and other Officers of Our Customs, the Master, Wardens and Company of *Stationers* of Our City of *LONDON*, and all other Our Officers and Ministers, whom it may concern, are to take Notice, that due Obedience be given to our Pleasure herein signified. Given at Our Court at *St. James's* the Seventh Day of *November*, 1737 in the Eleventh Year of our Reign.

By His MAJESTY's Command,

HOLLES NEWCASTLE.

MR COTES's

HYDROSTATICAL

AND

PNEUMATICAL

LECTURES.

LECTURE I.

That fluids gravitate in proprio loco, the upper parts continually pressing upon the lower; that this pressure is not only propagated downwards but even upwards and sideways according to all possible directions; that a lighter fluid may gravitate upon a heavier and a heavier upon a lighter.

BEFORE we begin our experiments it may be convenient to give some account of the method I intend to follow throughout the whole course, and to allot to each of the four weeks, in which I hope to finish my design, its distinct share of the tryals, by which I propose to make out and clear up those truths and conclusions in hydrostaticks and pneumatiks, which seem to be the most fundamental and of the chiefest importance. Hydrostaticks and pneumatiks have in nature so near a relation to each other, that they ought never to be separated: being therefore both comprehended in one course, they are disposed into that order which was judged to be the most natural for deducing one consequence from another which was proved before,

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most easy for performing the experiments, those being usually placed together which require a very little different *apparatus*, and most conformable to the succession of times in which the experiments and the truths established from them appeared in the world.

In this first week then I shall endeavour to settle the grounds of hydrostaticks, and to account for those various effects which depend upon the pressure of fluids against other fluid and solid bodies. This science as it began first to be cultivated and was brought to a considerable degree of perfection by the sagacity of the great *Archimedes*, so is it the most solid foundation upon which our modern philosophers have built their admirable doctrine of the air's pressure: which notion seems first to have been started about an hundred years ago by the famous *Galileo*, and was happily prosecuted by his scholar *Toricellius*, and after him by several of the most eminent *Virtuosi* of *Europe*. Whoever shall attentively consider the several phænomena which I shall this week prove to depend upon the gravitation of fluids in general, will find it no difficult matter, when we come afterwards to the air, which is a particular fluid, and which I shall prove to be not devoid of weight, as it was formerly thought to be, to explain by the gravitation of this subtle fluid those once surprising effects which so much puzzled the philosophers, and compelled them for concealing their ignorance to run into very great absurdities.

Having assigned to this first week the business of hydrostaticks, in the next I shall enter upon pneumatics, which furnish us with the knowledge of the nature and properties of the air. I need not here insist upon the usefulness of this science, since it cannot but be well known to every one, how much all natural philosophy depends upon it; there being no
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one body in the world that has a more universal influence upon the general course of nature than the air. The famous *Torricellian* experiment was that which first alarmed the philosophick world, and his mercurial tube has justly been as much celebrated for opening the way to pneumatical discoveries, as his master *Galilæo's* optick tube has been for the advancement it has made in astronomy. After *Torricellius* many *Italian, French* and *English* gentlemen made several other, for the most part, tubular experiments, to illustrate and confirm the doctrine of the air's pressure before the air-pump was invented. I have therefore in the second week proposed to exhibit those tubular experiments which were commonly made before the air-pump was used, adding to them some others of the like nature that have since that time been thought on.

In the third and fourth weeks I design to repeat the principal experiments that are wont to be tried with the air-pump and condenser, such of them at least as the *apparatus* of instruments which we have at present are capable to exhibit. For though our air-pump be as commodious as any that has yet been made, yet our condenser is not such as I could wish for, and I hope some time or other to be furnished with an engine which will perform both parts with more advantage than can be expected from our present instruments, and that the rest of our *apparatus* may be so enlarged as to afford us a greater plenty of experiments. In our third week then I intend to make such tryals as seem to be most pertinent for establishing the principal properties of the air, such as its weight, spring, pressure, resistance, refraction; explaining by these properties several remarkable effects. Now though some of these properties will be deduced from the tubular experiments of the foregoing week, yet I suppose that nobody will be displeas-

ed to see such things, which were once and not long ago very much controverted, made out by more ways than one.

To the fourth and last week I have allotted such experiments as shew some very notable effects of the air, which depend upon more hidden properties than those explained in the third week. Amongst these we may reckon the experiments upon the life of animals, upon flame, and others of that nature. And because several effects have been by philosophers ascribed to the air which do not belong to it, I design at the same time to confute those errors by matter of fact and experience. This is the method which I have proposed to myself in our present course of experiments.

Omitting then all further preliminaries, let us begin with hydrostaticks. The signification and reason of the name need not here be explained, nor need I tell you that by hydrostaticks is now commonly understood that part of natural philosophy which considers the equilibrium and pressure of fluids in general, though that word seems to be restrained to water, which is a particular fluid and the most obvious of all others, and by means of which we shall make out most of our following conclusions. For whatever can be proved by experiments and reason to belong to any one body which is both fluid and heavy, upon account of its being fluid and heavy, must belong to all other bodies which are fluid and heavy. If a due caution be observed, we may very safely be allowed to draw general conclusions from experiments, which cannot otherwise be made but upon particular bodies.

Fluidity and gravity being the qualities which are of great concern in hydrostaticks, it will not be amiss to say something concerning each of them in this place. It is beside our purpose to enquire into
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the causes of these two properties of bodies ; I shall only observe of fluids that they are such whose parts yield to any force exercised upon them, and by yielding are easily put in motion amongst one another. But whence this disposition arises of giving way to, and being moved by every the least impression is a problem not readily to be solved ; I had rather wholly pass it by than propose uncertainties about it. The famous *materia subtilis* has been a constant refuge to our modern philosophers as well in this as in all other difficulties, but that way of juggling does now at last very justly begin to be out of credit, and ought altogether to be laid aside, till sufficient reasons can be produced for its admittance.

Gravity is well known to be the endeavour and tendency of bodies towards the center of the earth. It is a property of so universal an extent that no one body in the world is yet known to be without it ; not air, which as I shall afterwards shew, may be weighed in the ballance ; nor fumes and vapours, which seem to be lighter than air itself by their ascending in it, as I shall prove by experiment ; and the excellent Mr. *Boyle* has found out ways to make even flame itself ponderable.

Now though most men without any difficulty allow thus much, that water and other fluids are really ponderous, and do actually gravitate when taken as a whole body, being convinced by their senses that a vessel weighs less when empty than when filled with any fluid, and weighs still more as it contains more of the fluid, yet notwithstanding this, many eminent men have found much difficulty to believe that the parts of fluids do gravitate *in proprio loco*, as they speak, or upon one another. It would be endless and tedious to go about to enumerate the several prejudices which have been the occasions of this error among the philosophers, whilst they have

chosen rather to oppose any truth which came in their way, than forsake the opinion however false they had once resolved to adhere to. But since this error if admitted must of necessity subvert the foundations of all hydrostaticks, and the contrary truth is that by which most of our following conclusions must be explained, I ought not to proceed any further till I have cleared up this matter.

It is evident from daily experience that in any fluid the weight of the whole is equal to the weight of all its parts, of what magnitude or number soever those parts are supposed to be; and if any part be taken from the whole the weight of the whole will be diminished by the weight of that part; if any part be added to the whole the weight of the whole will likewise be increased by the weight of the part which was added, and it seems from hence to be a reasonable conclusion that the weight of the whole is composed of the weights of the several parts, and that the parts do therefore gravitate even in the whole or, according to their way of expressing it, *in proprio loco*. Notwithstanding so obvious and necessary a deduction, the opposers of it have been subtle enough to elude the truth by a distinction which they have invented. They grant that the parts do by an united action cause the gravitation of the whole, but deny that they do singly and separately gravitate *in proprio loco*, so as to compose by that means the whole gravitation. Whenever it can be clearly made out that a number of agents may jointly produce an effect, whilst each singly contributes nothing to that effect, it may then be time to answer this subterfuge.

Notwithstanding so necessary and obvious a deduction, there have been two grand arguments usually produced against the doctrine we assert, which if answered will also render the rest of the objections

objections invalid, they being for the most part reducible to the one or other of those two. It is an experiment obvious to every body that a bucket full of water is lighter in the water than out of it, nor does it weigh more when full in the water, than when empty out of it; therefore they conclude that the water in the bucket because it is within water, its own element, does not gravitate. The other instance is taken from divers, who are said to feel no sensible pressure under water, though they often descend to very great depths; therefore they again conclude that the parts of water do not gravitate nor consequently cause any pressure *in proprio loco*. Now granting the matter of fact to be true in both these cases, though it may justly be questioned as to the business of diving, yet till they can prove that these matters of fact are no other way to be accounted for than by that which they have proposed, their inferences can by no means be allowed them. I shall take an opportunity whilst we are upon hydrostaticks, to give the true reason why the weight of the water in the bucket ought not to be perceived whilst the bucket is in water, though it do really at that time retain all its weight which it has when taken out of the water. And as to divers, though we allow that at the depth of thirty two feet under water, they have upon the surface of their whole bodies a more than ordinary pressure of twenty thousand pounds weight, yet when we consider the uniformity of that pressure and its equability, which causes no dislocation of parts, all the external being equally affected with it, and being internally supported by the air and other elastick fluids, which constantly endeavour the more to expand themselves as they are more compressed, when we also consider the firm texture of the membranes and other solid parts of human bodies and

the incredible force they are able to bear, as has been made evident by experiments, we shall not much wonder that divers complain of no sensible pain though they be certainly prest with so great a weight of water, besides the ordinary pressure of the air which our bodies are continually exposed to, which is equal at least to that of water at the depth of thirty two feet, or twenty thousand pounds: so that the whole pressure to which a diver is exposed at thirty two feet under water, is about forty thousand pounds. Since then we have proved that fluids do gravitate *in proprio loco*, and these difficulties do not destroy that assertion, as the proposers of them would make us believe, when they tell us they cannot otherwise be explained, what has been hitherto said might be taken for a satisfactory answer to the objections; yet because this truth is of so great moment I will endeavour to confirm it by two experiments levelled against those two objections, shewing by the first of them that fluids lose nothing of their weight *in proprio loco* (a) and by the second that the lower parts of fluids are pressed by the upper, and commu-

(a) Fluids lose nothing of their weight *in proprio loco*.

EXP. I. FIG. I, represents a roundish glass bottle, thick enough when empty to sink in water, with a stop-cock cemented to the mouth of it; whereby a quantity of air may be taken out of the bottle by an air-pump, and be hindered from returning by shutting the cock; in order to give passage and room to a quantity of water, equal in bulk to the exhausted air, as soon as the cock shall be opened under water contained in a larger vessel.

The air being exhausted, let the bottle be suspended by a wire to the beam or scale of a ballance, and let it be exactly counterpoised in the air by the weight A in the opposite scale.

Again when the bottle is suspended in a vessel full of water and wholly immersed, let it be counterpoised by a weight B, after A is taken away and reserved.

Then having opened the cock under water, to let it run into the bottle, after the running is over let a third weight C together with B restore the equilibrium of the ballance.

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communicate that pressure to bodies exposed to their contact. I will also add other experiments to prove that the pressure caused by the gravitation of fluids, is not only propagated downwards but even upwards and sideways according to all possible directions, (b) that a lighter fluid may gravitate upon a heavier, and a heavier upon a lighter. (c)

It is plain then that C is the weight of the water let into the bottle, even while it communicates with the water in the vessel.

Having shut up this water in the bottle by turning the cock while under water, take the bottle out of the water, and while it hangs in air at the beam of the ballance, take the weight B from the opposite scale and restore A in its stead, then will the weights A and C exactly counterpoise the bottle and the water within it.

Which shews that this water weighs just as much in air as it did before (*in proprio loco* or) in the water of the vessel, the cock being open.

(b) Pressure is caused by the gravitation of a fluid and is propagated every way alike.

EXP. 2. FIG. 2, 3, 4, represent tubes bent near their lower ends into various angles. They were first filled at their lower orifices with quicksilver, which rested in the longer legs upon a level with those orifices, and then were dipped into a deep glass vessel filled with water, which while the tubes were descending, gradually pressed the quicksilver from the lower orifices towards the higher, where the water could not enter.

To shew that pressure is propagated even upwards as well as all other ways, dip an open end of a very narrow-bored tube into quicksilver, then stopping the upper end with your finger, lift up the tube, and a short column of the quicksilver will hang in the lower end, which column when dipped deeper into water than about fourteen times its own length, will not only be suspended but even pressed upwards, after your finger is removed from the upper orifice.

(c) A lighter fluid may gravitate upon a heavier, and a heavier upon a lighter. The first part of this proposition has just been proved by the foregoing experiments, and if any oyl that is lighter than water, be put into the lower end of a bended tube, it will also be depressed by the water in the vessel, and forced from the lower orifice towards the higher.

LECTURE II.

That a fluid may sustain a body heavier in specie than itself and even raise it up, that a fluid may detain a body lighter in specie than itself and even depress it, that a competent pressure of a fluid may produce the remarkable phænomena of the Torricellian tube, pump, syringe, syphon, polished plates and other effects of the like nature.

Yesterday it was proved that fluids retain their gravitation *in proprio loco*, and by that gravitation pressed upon bodies exposed to their contact, that this pressure is not only propagated downwards, according to the tendency of heavy bodies, but also upwards and sideways according to all manner of directions.

I design this day to make some farther experiments concerning the pressure of fluids, by which I shall endeavour to demonstrate some of its more general effects; reserving the particular and exact estimate of all manner of pressures to my next lecture. In the choice of this day's experiments I have had regard to some of the most obvious and notable phænomena, that are now a-days explained by the air's gravitation; such as the strong cohesion of polished plates, the suspension of quicksilver in weather-glasses, the effects of syringes, pumps and syphons. If we can make it appear, that these things not only may depend upon the gravitation of a fluid, but must necessarily be the consequence of such a gravitation, and can afterwards prove, that the air itself is a gravitating fluid, and can determine the proportion of its specifick weight to that of any other fluid, it will be no difficult matter from these things laid together, to evince directly, that the pressure of the air is the cause of those phænomena, and several others of

of the like nature. Let us then consult experience, and try whether any thing analogous to what I have been mentioning, be the result of that pressure, which is caused by the gravitation of fluids (*a*).

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(*a*) Here the author made the following experiments and explained them in the sequel of his lecture.

EXP. 1. In FIG. 7. *ab* represents a large round plate of thick brass, whose upper surface being covered with wet leather, is applied so close to the orifice *cd* of the inner vessel, as to hinder the entrance of the water contained in the outer vessel. Now if this plate be held tight against the said orifice, by pulling the wire *e* fixed to the plate, till it be immersed to a sufficient depth of water in the outer vessel, the plate will be supported by the pressure of the water acting upwards, though the hand be taken from the wire. The margin of the inner vessel is made broad enough to hang upon the margin of the outer.

EXP. 2. has already been described, *Page 9. Note (b) Sect. 3.*

EXP. 3. FIG. 8, represents a small glass cup having a flat wooden bottom, well planed and fixed to the glass with cement. Upon this bottom place a thick wooden plate, whose under surface is also well planed, and press it against the bottom with your fingers while quicksilver is poured into the cup so as almost to fill it. Then take away your fingers, and the wooden plate will still be detained at the bottom, until you disjoin it by pulling the pin fixed to the middle of it.

EXP. 4. FIG. 9. Any sort of oyl lighter than water, is poured into the shorter leg of a tube bent parallel to the longer, then while the tube descends gradually into a vessel full of water, the oyl will descend into the shorter leg and ascend in the longer.

EXP. 5. Two cylindrical cups containing tinged water, whose surface is about an inch higher in one than in the other, are placed within a larger glass vessel, whose bottom is levelled with bees wax that the cups may stick to it and stand upright. Into the tinged water in the cups put the legs of a glass siphon, having an open pipe inserted into the middle of it, as represented in FIG. 11, and put a wooden cover over the vessel, having a hole in its center to receive this pipe and keep it upright; then through a funnel, inserted into another hole in the cover, pour oyl of turpentine into the larger vessel, till it flows into the cups and rises above the arch of the siphon. The pressure of the oyl upon the tinged water in the cups, will cause the water to pass through the siphon from the higher cup to the lower, till the surfaces of the water be reduced to a level in both the cups.

EXP. 6. The last experiment being easily explicable, is applied to explain the effect of a common siphon, which is nothing else but

These experiments will need something of an explication; I shall therefore give the reason why the events ought to be such as we have seen, and afterwards infer those consequences which I told you were the principal motives that induced me to make these tryals.

The cause of the firm adherence of the brass plate to the orifice of the glass to which it was applied, is this; that the parts of the water immediately contiguous to the under surface of the plate, were very much pressed by the water which was above the level of the under surface of the plate, and which surrounded the glass to whose orifice the plate was applied, that superior and ambient water communicating a pressure downwards to those parts upon the same level which were directly under it, and these again laterally communicating the pressure they had received, to the other parts upon the same level which were immediately under and contiguous to the plate. For if the parts of the water contiguous to the plate were not as much pressed as the others upon the same level, which were directly under the ambient water, they could not by reaction be able to sustain the pressure of the others, but would give way to the admittance of a further pressure, which must for this very reason be increased till both came to an equipollency. The water immediately contiguous to the under surface of the plate being thus pressed, by communicating its pressure upwards causes this strong union of the plate and the orifice of the glass, notwithstanding the weight of the plate by which it endeavours to descend and to be disunited; which we

but a bended pipe, whose shorter leg being put into any liquor and your mouth being applied to the longer, to suck up some liquor, the rest will continue to ascend in the shorter leg and descend through the longer, till the whole be exhausted from the vessel.

saw it actually did effect when it was not so deeply immersed, and had not so great an altitude of water as was able to cause a sufficient resistance.

Now to know what depth of water is able to cause a sufficient resistance, we ought to know the weight of the plate and whatever else is annexed to it, as in our case the wire and the leather which covers its upper side. We must then immerse it so deep at least that the perpendicular distance of the under surface of the plate and upper surface of the ambient water, be equal to the height of a column of water, whose base is equal to the under surface of plate, and weight equal to the weight of the plate with whatever is annexed to it. For then the pressure of the water against the plate will be a ballance to the weight of the plate against the water, as may (among other ways) be thus made out.

Every part of water which is directly under and contiguous to the plate, receives and communicates a pressure equipollent to that of every equal part of water upon the same level, which is directly under the ambient water, as hath been proved before; now these parts which are under the ambient water receive their pressure from the weight of the column of water, which is perpendicularly incumbent on them, and by their reaction sustain that weight, therefore the pressure which every one of these parts receives and communicates, is equipollent to the weight of its respective superior column; since then the parts of water contiguous to the plate, which are equal to those we have been speaking of, receive and communicate the same degree of pressure, the pressure of these also will be equipollent to the weight of a like column of water; and the sum of their pressures or the force with which they do unitedly sustain the plate, will be equipollent to so many such columns as there are parts of water contiguous to the

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the under surface of the plate. Now all those columns together are equal to a cylinder or column having that under surface of the plate for its base, and the perpendicular distance of that under surface of the plate and upper surface of the water for its altitude. If therefore the weight of that cylinder be any thing greater than the weight of the plate, the plate will be sustained; if the plate be not so deeply immersed that the weight of the cylinder be at least equal to the weight of the plate, the plate will be disjoined, by its excess of weight, from the orifice of the glass; if it be deeper immersed, the pressure of the water will by its excess of weight, be more than sufficient to sustain it.

The specifick gravity of brass to that of water is nearly as eight is to one, I mean that bulk for bulk brass is about eight times heavier than water; therefore the weight of a cylinder of water will be equal to the weight of a cylinder of brass, if their bases be equal and the altitude of the water be eight times as great as the altitude of the brass. Hence we may conclude that our plate which is of brass, ought to be immersed under water at least eight times its thickness, to be supported by the water. For the same reason a plate of pure gold, which is the most ponderous body we meet with, would require near twenty times its thickness. Upon this account it was that Mr. Boyle proposed one of his hydrostatical paradoxes in these words, *That a solid body as ponderous as any yet known, though near the top of the water it will sink by its own weight, yet if it be placed at a greater depth than that of twenty times its thickness, it will not sink, if its descent be not assisted by the weight of the incumbent water.*

The other experiment in which the quicksilver which is yet heavier than brass, was sustained by water, which is a fluid about fourteen times specifically

fically lighter, is explicable the same way. I will therefore only take notice that, whereas I asserted before that an heavier body immersed in a fluid may be either immersed so deep as to be just sustained, or more than just sustained, or so as to be not quite sustained, according to the different weights of the body to be sustained and the column of the fluid that was before described, the truth of the assertion is in this experiment very manifest. For when we immersed the quicksilver to a just depth, we saw it rest in the pipe without either ascending or descending; if the pipe were thrust deeper, the quicksilver in it was impelled upwards by the force of a more than sufficient pressure of the fluid; if the pipe were raised up above that just depth, the quicksilver by its excess of weight would in part fall out. This experiment will never succeed unless the pipe which contains the quicksilver be of a very narrow bore; for if it be not so, the water will get a passage by the side of the quicksilver, and this changing of places will soon frustrate the event.

The experiment of the wooden plate remaining at the bottom of the vessel filled with quicksilver, would possibly appear strange to some who are unacquainted with the true principles of hydrostatics, and prejudiced with the false notion of positive, real or absolute levity. For if that imaginary levity were indeed the cause why light bodies ascend in fluids less light than themselves, we should be utterly at a loss in explaining this phænomenon. For what reason can we assign why the wooden plate by the force of its imagined levity should not in this case, as well as in all others, make its way through the body of the quicksilver, which is by so great odds less light than itself. But if this positive levity be rejected as we shall afterwards give further proof that it ought to be (there being no such thing in nature as a positive

tive levity of bodies) and the gravitation of fluids be admitted, which we have already shewn to be consonant to reason and experience, this difficulty will instantly vanish. For it cannot but be evident that the plate ought always to remain at the bottom, unless it be displaced and impelled upwards by some adventitious force, which in our case does not happen, the quicksilver not being able to insinuate itself between the plate and the bottom upon which it rests, nor consequently by its pressure upwards, against the under surface of the plate, to remove it to the top, which we saw it did immediately effect when we permitted it to intervene. It would be yet more difficult from the principle of positive levity to give any account why the plate does not only emerge of itself, but requires even a considerable force to disjoin it from the bottom. It is manifest that this cannot otherwise be accounted for, but must be ascribed to the gravitation and pressure of the quicksilver incumbent upon the plate. The force requisite for their separation being greater or lesser according as there is a greater or lesser depth of quicksilver to hinder their separation. It may perhaps seem more strange to some, as it was a most perplexing difficulty to the famous Dr. *More*, that immediately upon their separation there should be not only no further need of any force to raise up the plate to the top, but that of itself it should very violently emerge; whereas immediately upon their separation, when it first begins to ascend, there is still almost as great a weight of incumbent quicksilver to depress it, as before the separation there was to detain it. If we say it is raised upwards by the quicksilver which intervenes upon the separation, and that this intervening quicksilver receives the pressure, which it communicates upwards to the wooden plate, from the rest of the quicksilver in
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the vessel, which is not perpendicularly incumbent on the plate, but which furrounds that incumbent quicksilver, being contiguous to the sides of the vessel, and resting upon that annular part of the bottom, which is not covered by the plate; the Doctor will here urge a further difficulty, and will tell us, that the pressure upwards caused by the weight of the ambient quicksilver can by no means equal, much less exceed, the pressure of the incumbent (besides the weight of the plate) that being in quantity much less than this; so that here he supposes he has found out some work for his *Principium Hylarchicum* or his Spirit of Nature.

What has been already said in the explication of the first experiment, may be a sufficient solution of this difficulty; for it was there proved that the force with which the brass plate was suspended, was equal to the weight of a cylinder of the fluid in which it was immersed, whose base was equal to the area of the plate, and altitude equal to the perpendicular distance of the under surface of the plate and upper surface of the fluid, of what breadth soever the ambient fluid were supposed to be: which paradox shall be further explained and experimentally illustrated at our next meeting. In our present case then, the force which impells the wood upwards, is equal to the weight of a cylinder of quicksilver having the under surface of the wood for its base, and the perpendicular depth of that under surface for its altitude; which weight does evidently exceed that of the incumbent quicksilver and plate together, just as much as the weight of the wooden plate itself falls short of the weight of an equal bulk of quicksilver. Quicksilver is fitter to be used in this experiment than water, because it does not adhere to or wet wood as water does, and cannot therefore so easily insinuate itself between the plate and the bottom upon which it rests,

The next experiment that was tryed, was proposed to evince the same thing with this last, that fluids may detain and even depress a specifically lighter body than themselves; there was only this difference, that in the former the specifically lighter body was a solid, and in the latter a fluid. I shall pass over the explication of the latter, presuming that by this time the reason of it is sufficiently obvious.

You may remember we immersed two pipes of different bores in a vessel of quicksilver, FIG. 10, then pouring on water which could not get into the pipes, their upper orifices being above the water and their lower under the quicksilver, the quicksilver was seen to ascend in the pipes above the level of that in the vessel to the same altitude in both. This effect is an easy and plain consequence of the gravitation and pressure of water upon the quicksilver. For if we imagine a plane, parallel to the horizon, to pass by the under orifices of the pipes, it is certain that these fluids cannot rest in any position till every equal part of this imaginary plain doth sustain an equal pressure with the rest, as has been proved before; now the parts of this plane which are directly under the orifices of the pipes, cannot sustain an equal pressure with the rest, unless the quicksilver be supposed to ascend so much in the pipes as to ballance that excess of water which presses upon the other parts; and although it might seem at first sight that a lesser height of quicksilver would be sufficient for this purpose in the larger pipe than in the smaller, a lesser height of quicksilver in that being equiponderant to a greater in this, yet in reality it is quite otherwise, and the heights in both ought to be equal. For though there is a greater weight of quicksilver in the larger pipe, yet we must at the same time consider, that this greater weight has a proportionably greater part of the imaginary plane to communicate a pressure to,

We are now come to that ingenious experiment first proposed by M. *Pascal* to manifest that the effect of a syphon may depend upon the gravitation of a fluid. We saw it with our eyes and can therefore no longer doubt of it, that the weight and pressure of the oyl caused the tinged water to take its course from the higher vessel into the lower through the bended pipe. But that you may have the evidence of reason as well as of sense, I will give you that excellent author's own explication of this matter, after I have told you that he instead of our tinged water made use of quicksilver, and instead of our oyl of turpentine he tried his experiment with water. We are to observe, says he, that the oyl gravitating upon the tinged water contained in each vessel, and not all upon that which is contained in the legs of the syphon, it comes to pass that the water in the vessel is compelled by the weight of the oyl, to ascend in each leg to the top of the syphon, and there a sort of conflict must happen betwixt the two ascending columns, each pressing against the other, and that will necessarily prevail which has the greater force. Now which has the greater force may easily be determined, for since the oyl has the greater altitude above the lower vessel by an inch, it must more powerfully elevate the water of the longer leg, than that of the shorter by the force which an inch of altitude gives to it. Whence it seems at first sight to follow, that the water ought to run from the longer to the shorter leg. But we must at the same time consider that the weight of the water in each leg resists the effort of the oyl to raise it up, but both do not resist equally, for the water in the longer leg has a greater altitude by an inch, and so makes a greater resistance by the force which an inch can give it. In the longer leg the water is more powerfully elevated by the force of an inch of oyl,

and its ascent is more powerfully hindered by the force of an inch of water; now an inch of water is more ponderous than an inch of oyl; therefore the water of the shorter leg absolutely speaking is elevated with a greater force, and consequently it ought to ascend, and continue to ascend till the water in both vessels comes to a level. (*b*)

From hence it appears that the reason of the higher vessel's emptying itself into the lower, is that the water is heavier *in specie* than the oyl. The contrary would happen if the syphon and the vessels into which it is immersed were filled with oyl, and all were immersed in a vessel of water; for then it would come to pass that the oyl of the lower vessel would ascend, and passing by the top of the syphon would descend into the upper vessel, upon the account which I have just now mentioned. For the water pressing continually upon the oyl in the lower vessel with a greater force, since it has by an inch a greater altitude, and the oyl of the lower leg gravitating and resisting more by an inch of altitude, it must needs come to pass, since an inch of oyl weighs less than an inch of water, that the oyl of the lower leg ought to be more forcibly elevated than the oyl of the higher, and therefore the course must be from the lower vessel into the higher. Upon the same account if the syphon were filled with a liquor of the same gravity with water, no flux would ensue but all things would remain at rest.

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(*b*) The author has here given us M. *Pascal's* explication of his own experiment, but in common discourse I remember he explained it as follows.

In FIG. 11. Supposing the legs of the syphon to be really equal, or which comes to the same, supposing an horizontal plane to pass through the legs and tinged water in both cups, the parts of this plane within the legs will be equally pressed by equal columns of tinged

From these experiments I might now in a few words and very easily deduce those inferences, for whose sake they were chiefly proposed, were they not already too obvious to be insisted on. I will therefore only mention them. Supposing then the air to be an heavy fluid, and that the surface of the earth is as much pressed upon by this fluid as if it were every where covered with quicksilver to the height of about 29 inches and an half, or with water to the height of about 34 feet, as we shall hereafter prove; if in the first experiment, FIG. 7, we substitute air instead of water, and instead of the plate of brass applied to the orifice of the glass vessel, two polished planes applied together so closely as to exclude the air from getting between, the lower plane must of necessity be pressed against the upper and kept suspended. So in the last but one of these experiments, in which the quicksilver was raised above its level in the two pipes, FIG. 10, if we substitute the body of a pump for either of those pipes, water in the well for quicksilver in the vessel, air incumbent upon the water in the well for water incumbent upon the quicksilver in the vessel, and observe that as the water in our experiment was hindered from entering into the

tinged water within the legs; but other equal parts of this plane on the outsides of each leg, will be unequally pressed by their incumbent columns though of equal altitudes; because the columns of the higher cup consist of more water and less oyl than those of the lower. The heavier columns will therefore press up the higher water into the leg in its cup, with greater force than the lighter columns can press up the lower water into the leg in its cup, and the excess of the former pressure above the latter will drive the water along the syphon from the higher cup to the lower.

This excess of pressure, which causes the flux, is therefore as the difference in weight of two columns composed of water and oyl, whose common base is equal to the orifice of the syphon, and common height is the difference of the heights of the water in the cups.

pipe by the sides of the pipe, so the air is excluded from the cavity of a pump by the sides of the pump and the sucker; it will be clear to any one, that water may ascend, by the pressure of the external air upon the surface of the water in the well, to the height of about 34 feet. The syringe is a little pump as the pump is a greater syringe, what has been said of the pump may therefore be applied to the syringe. The case is very nearly the same with the Torricellian tube, barometer, or weatherglass, in which the quicksilver usually ascends to 29 inches and an half; that height of quicksilver being equiponderant to 34 feet of water. This also may be taken notice of, that as in our experiment the quicksilver ascended to the same height in both pipes, though of unequal diameters, so in pumps and barometers the altitude of the liquors is not altered by any difference of their bores. The last experiment is so particularly fitted to the common syphon that any one may make the application.

LECTURE III.

That fluids press according to their perpendicular altitudes, whatever be their quantities or however the containing vessels be figured; the exact estimate of all manner of pressures; and the invention of the center of pressure upon any proposed plane reduced to the problem of finding the center of percussion,

WE are now to determine the quantity of that pressure which any surface sustains that is exposed to the gravitation of a fluid: this must be done gradually, beginning with those cases which are most simple and easy, and afterwards proceeding to those which are more complex and difficult. Let a vessel *abcd* FIG. 12. be proposed containing any fluid, suppose water,

water, and let ab be the upper surface of the water, and cd the bottom of the vessel. The pressure upon any part of that bottom, suppose gb , will be equivalent to the weight of a column of water $gbik$, having the part gb for its base and gi or bk , the depth under water, for its altitude. This seems to be self-evident and may best be proved by the absurdity of any contrary supposition; for if it be said that gb sustains a greater weight than that of the column $gbik$, the excess must come from the adjoining columns $acgi$ and $kbdb$; now for the like reason it ought to be said that cg sustains a greater weight than that of the column $acgi$, and bd a greater than that of the column $kbdb$; but if this were true, then would all the parts cg , gb and bd together, of that whole plane cd , sustain a greater weight than that of the columns together, or of the whole water which is above it, namely $acdb$, which is absurd. The like absurdity will follow if it be said that gb sustains a lesser pressure than the weight of the column $gbik$; the weight of that column then which is perpendicularly incumbent upon it, is exactly equivalent to the pressure which it sustains.

This is the quantity of pressure upon the plane gb in the case that has already been described. If the figure of the vessel be any way altered, the pressure will still be the same if the perpendicular distance of the plane gb from the upper surface of the water contained in the vessel, of whatever figure it be, remain unaltered. Thus in FIG. 13, 14, if $l ngbom$ be a vessel of any irregular figure, and lm be the upper surface of the water, and the perpendicular distance of gb below lm namely gi or bk be the same as before, the pressure of the vesselled water $l ngbom$ upon the bottom gb will be equal to the same weight of the column $gbik$ as before, though the vesselled water $lgbm$ be much less than that column, as in

FIG. 13, or much greater, as in FIG. 14. The pressure is not to be estimated by the quantity of water but by its altitude. For if the quantity of water $lgbm$ in FIG. 13, be a thousand times less than $igbk$, as we may easily suppose it to be, and the quantity of water $lgbm$ in FIG. 14, be a thousand times greater than the same $igbk$, then the quantity of this latter will be a million of times greater than that of the former, nevertheless both will equally press upon their bottoms gb with a force equivalent to the weight of the perpendicular column $gbik$; which may deservedly be accounted a paradox in hydrostaticks, but may thus, among other ways, be rendered intelligible.

Let us conceive each of those vessels placed in a larger $abcd$; the pressure upon gb will be the same whether we suppose the water $lgbm$ to be contained in its proper vessel $lgbm$, or, imagining that vessel to be away, we suppose its place to be supplied by the ambient water $acgnl$ and $hombd$; for any parcel of water may be conceived to be kept in by the rest of the water, which every way surrounds it as in a vessel, supposing all things at rest. Now in this latter case where we suppose the ambient water $acgnl$ and $hombd$ to be a vessel to the water $lgbm$, the pressure upon gb is equivalent to the weight of the column $gbik$, as has been already made out; therefore in the former case, where the water $lgbm$ was contained in its proper vessel, the pressure upon gb will be also equivalent to the weight of the same column $gbik$. By the same way of reasoning we may conclude that the water contained in any other more irregular vessel, as $lgbm$ in FIG. 15, presses upon the bottom with a force equivalent to the weight of the column of water $gbik$, having the said bottom for its basis, and gi or bk , the perpendicular distance of the planes gb and lm , for its altitude.

If the plane $g b$ be oblique to the horizon, as in FIG. 16, the pressure upon $g b$ from the water of the vessel $l n g b o m$, or from that of the vessel $e g b f$, or from that of the larger vessel $a c d b$, will still be the same, if the upper surfaces $l m$, $e f$ and $a b$ be in the same plane or at the same altitude above $g b$. The altitude is every where the measure of pressure whatever be the quantity of the fluid, or however the containing vessel be figured (*a*).

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(*a*) The author gave an experimental proof of these conclusions as follows. FIG. 17 represents a large mouthed syphon inverted and partly filled with water, whose surface always rests at the same level in both legs; consequently supposing the syphon to be cut at the bottom of the flexure by an imaginary plane, the water in both legs, however different in shape and bulk, presses with equal and opposite forces against that plane; otherwise the level of the water would soon be altered and destroyed.

It has been shewn above, that the brass plate $a b$, in FIG. 7, being about eight times heavier than an equal bulk of water, will by the pressure of the water underneath it, be supported at the mouth of the inner vessel, if the plate be immersed under water above 8 times its thickness. This being done, let other water be gradually poured through a funnel into the inner vessel, till its weight and pressure shall cause the plate to descend, and at that instant let the altitude of the inner water above the plate be observed, or rather the difference between the altitudes of the inner and outer water, which difference by theory should be about eight times the thickness of the plate.

This being observed, take up the inner vessel, and into its mouth squeeze a large cork $c d$ with a wet leather bound about it, so far as to leave a thin space between it and the plate applied to the orifice of the mouth, as represented in FIG. 18. A glass tube $g b$ was first squeezed through a hole in the middle of this cork and cemented, to it, and then the wire $c f$ fixed in the plate was put through the tube. Then while the plate is held by the end of the wire against the orifice of the inner vessel, the whole is let down into the water in the outer vessel, by whose pressure upwards the plate is again supported, as before. But by degrees the ambient water will insinuate itself between the orifice and the plate, into the thin space above it, and this being filled, it will quickly rise into the tube, where as soon as it arrives at the altitude before observed in the whole inner vessel, its weight and pressure will immediately cause the

I should now proceed to estimate the pressure upon planes which are either perpendicular or oblique to the horizon, but because the several indefinitely small parts, of which such planes are composed, are acted upon with different forces, accordingly as the particles of water, by which they are immediately touched, happen to be at different depths; and since the total pressure is made up of all these different forces taken together, we ought before we go any further to consider, what will be the pressure which each of these indefinitely small parts sustains. First then we are to consider that every small particle of water, which is at rest, is pressed upon equally on all sides by the other particles which surround it, otherwise it would yield to the stronger force till it were equally pressed every where; and as it is equally pressed on all sides, so does it every way by reaction equally press whatever is contiguous to it, according to all possible contrary directions; for should it press less than it were pressed, it must necessarily yield to the force which is supposed greater than its own; and should it press more than it were pressed, its force would necessarily remove its weaker antagonist. Therefore since all things are supposed to be at rest, we cannot any ways imagine this inequality of pressure to take place. Now it has been proved before, that the pressure from above is equivalent to the weight of the incumbent column of water, therefore the pressure from any other part, or according to any other direction, is also equal to the weight of the same incumbent column; and since action and reaction are equal, the particle itself must press according to all manner of directions with the same force, which is equivalent to the weight of the incumbent column.

the plate to descend: which shews that the pressure of this smaller quantity of water is equal to that of the larger upon the same base.

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It is evident then that as fluids press according to all possible directions, so are the pressures equal according to all directions, if the points of contact in which the pressures are made be at equal depths. This being allowed we may proceed to what remains.

Supposing then that $acdb$ in FIG. 19, is a cubical vessel in which the water reaches to the top, so that its upper surface be represented by ab , let it be required to determine the pressure which one of its sides ac sustains from the included water. This side ac though represented here by a line, to avoid confusion in the scheme, is supposed to be a square. The measure of the pressure upon every physical point of that square, or as it is here represented, of that line ac , is the altitude of the water above that point; thus the pressure upon l is measured by al , the pressure upon m by am , the pressure upon n by an , and the pressure upon c by ac , and the same may be said for any other points of the line ac ; therefore the pressure upon the whole line, or upon all the points of it, will be measured by the sum of so many of those altitudes al , am , an , ac , as there are points in the line ac . Now that sum may be estimated by drawing the perpendicular lo equal to la from the point l , the perpendicular mp equal to ma from the point m , the perpendicular nq equal to na from the point n , and the perpendicular cd equal to ca from the point c . Now it is evident that the sum of al , am , an , ac must be equal to the sum of lo , mp , nq , cd , and if from every intermediate point between a and l , l and m , m and n , n and c , perpendiculars be conceived to be drawn after the same method, the sum of all those perpendiculars will be the measure of the total pressure upon the line ac . But the sum of all those perpendiculars is equal to the area of the triangle acd , therefore the area of the triangle acd is the measure of the pressure upon the line ac .

Now

Now as the line ac represents a square, so will the triangle acd represent a prism, having the said triangle for its base, and the side of the square for its altitude. The weight of that prism of water is therefore equivalent to the pressure made against the square, or side of the cube. That prism is equal to half the whole cube, as we learn from Euclid's elements, therefore the pressure against the square is equivalent to half the weight of the whole water contained in the vessel. There are four such sides of a cube besides the top and bottom, and each of those four sides for the same reason sustains the same pressure, therefore all together sustain four times half the weight, that is twice the whole weight of the water. And the bottom, by what has been proved above, does itself sustain a pressure equal to the whole weight of the water; therefore the bottom and sides together of a cubical vessel filled with water, sustain a pressure from the water equal to thrice the weight of it.

I have endeavoured to make the thing as easy as I believe the nature of it will permit, however since that part of this deduction where I told you the triangle acd did at the same time represent the prism when the line ac represented the square, might be perhaps a little obscure, I will endeavour to clear up this matter something further. Let then $acfe$ in FIG. 20 represent the square side of the vessel, and $cdgf$ represent the square bottom of the same. It was proved before that the pressure exercised upon the line ac was measured by the triangle acd ; by the same way of reasoning it may be proved that the pressure upon the line ef is measured by the triangle efg , and the pressure upon any other line bi , which is parallel to these two and situated between them, is measured by its respective triangle bik . If we imagine the square $acfe$ to be made up of an infinite number of such intermediate lines as bi , the pressure

sure upon the whole square will be made up of the same infinite number of such equal triangles as bik ; now the sum of all those triangles make up the prism $aegdcf$, and this prism is half the whole cube, as in the former scheme the triangle acd is half the square $acdb$. If the plane $acfē$ instead of being a square were a rectangled parallelogram, having its side ae either longer or shorter than ac , it would follow from the principles, that the pressure to which it is exposed, would be equivalent to the weight of a like prism of water having the triangle acd for its base, and the side ae for its altitude.

I have been hitherto speaking of planes which are either parallel or perpendicular to the horizon; it will be no difficult matter to apply what has been said of perpendicular planes to those which are oblique. Let ac in FIG. 21 represent any such oblique plane, and let the upper surface of the water be ab . The measure of the pressure upon the point l is ls the altitude of the water above that point, so tm is the measure of the pressure upon m , vn the measure of the pressure upon n , and xc the measure of the pressure upon c . Erect the perpendiculars lo , mp , nq , cr equal respectively to ls , mt , nv , cx , and imagine the like construction to be made for all the other points of the line ac , and the sum of all those perpendiculars, that is the triangle acr , will be the measure of the pressure upon the whole line ac . If this line ac be supposed to represent a parallelogram as before, then the triangle acr will as before become a prism, and the weight of that prism of water, which we are taught by Euclid how to measure, will be the pressure sustained by the parallelogram.

I have hitherto supposed that the line ca or the parallelogram represented by it, coincides with the surface of the water at a ; if that does not happen, but the highest part of the line or parallelogram is at
some

some distance from the surface, a computation of the pressure will still be easy enough. Suppose mc in FIG. 21, were the line or parallelogram proposed; the pressure upon the line mc will be measured by the trapezium or four sided figure $mcrp$, and the pressure upon the parallelogram represented by that line, will be a prism having that trapezium for its base, and the other side of the parallelogram, which is supposed parallel to the surface of the water, for its altitude.

From what has been said of these few particular instances we may now understand, that the pressure upon any plane of whatever figure and situation, is equivalent to the weight of a solid of water, which is formed by erecting perpendiculars upon every point of the plane proposed, equal to the respective distances of those points from the upper surface of the water. For the perpendiculars being the measure of the pressure upon the points from which they are erected, the sum of these perpendiculars, or the solid formed by them, will be equal to the sum of the pressures upon the points, or the total pressure upon the whole plane.

Or we may thus express the same thing after another way, and so take in all curved surfaces as well as planes; that the pressure upon any surface is equal to the sum of all the products which are made by multiplying every indefinitely small part of the surface into its distance from the top of the water. For the pressure upon each of those parts is equal to a column of water having the part for its base, and the distance from the top of the water for its altitude; and every one knows who has the least skill in geometry, that those columns are measured by multiplying their bases by their altitudes; therefore the sum of the products of all those bases or little parts by their altitudes, or respective distances from the
top

top of the water, will be equal to all the columns upon every little part, and therefore to a body of water whose weight will be equivalent to the total pressure upon the whole surface.

Now to find the sum of all these products, or a body of water equal to that sum, is a very difficult problem in most cases. *Stevinus* in his hydrostaticks has attempted it only in a few instances, and those of plane surfaces, and among plane surfaces he meddles only with such which he calls regular, nevertheless he has gone the farthest in this matter of any writer I have met with. To supply then this defect I will here lay down another rule, which is not only universal, but also as easy and expeditious as can be desired.

It is this ; the pressure upon any surface whatever, however it be situated, is equal to the weight of a body of water whose magnitude is found by multiplying the surface proposed into the depth of its center of gravity under water. So the pressure upon any number of surfaces of different bodies, however differently situated, is equal to the weight of a body of water whose magnitude is found by multiplying the sum of all those surfaces into the depth of their common center of gravity under water.

The demonstration of this rule may not perhaps be fully understood by those who are unacquainted with staticks and the nature of the center of gravity, however I will here produce it, that those who can, may understand it, and that others, taking now for true what I shall assume as demonstrated by the writers of mechanicks, may afterwards be fully satisfied of it, when they come to understand the theorem it is grounded upon ; which is, that if every indefinitely small part of any surface, or number of surfaces, be multiplied respectively into its perpendicular distance from any proposed plane, the sum of those products will be equal to the product of

the whole surface or number of surfaces multiplied into the perpendicular distance of the center of gravity of the single surface, or of the common center of gravity of the whole number of surfaces, from the same plane. (*b*)

Now taking the upper surface of water for that plane to which we refer the indefinitely small parts

(*b*) In FIG. 22, let any number of quantities a, b, c, d , represent as many weights, hanging at their centers of gravity a, b, c, d , by the lines ao, bo, co, do , fixed to any horizontal plane o, o, o, o ; and let z be the common center of gravity of all the weights, and zo its perpendicular distance from that plane; I say that $a \times ao + b \times bo + c \times co + d \times do = a + b + c + d \times zo$.

For let the common center of gravity of the weights a, b be the point x , and to the line zo drawn parallel to the rest, let am and bn be perpendiculars. Then by the similar triangles mxa, nxb , we have $mx : nx :: (xa : xb ::) b : a$ by the known property of a center of gravity. Hence $a \times mx = b \times nx$, or $a \times mo - xo = b \times xo - no$, or, $a \times mo - a \times xo = b \times xo - b \times no$, whence $a \times mo + b \times no = a + b \times xo$; which was to be proved in the simplest case of the proposition.

Now let a weight $x = a + b$ be suspended by a line xo in the common center of gravity of a and b , and likewise a weight $y = x + c$ in the common center of gravity of x and c , and also a weight $z = y + d$ in the common center of gravity of y and d . Then is z the common center of gravity of all the weight a, b, c, d , first proposed.

Consequently by what has been proved in the first case, we have $a \times ao + b \times bo = x \times xo$, and likewise $x \times xo + c \times co = y \times yo$, and likewise $y \times yo + d \times do = z \times zo$; consequently $a \times ao + b \times bo + c \times co = y \times yo$, and likewise $a \times ao + b \times bo + c \times co + d \times do = (z \times zo =) a + b + c + d \times zo$, which was to be proved.

Hence if a surface or number of surfaces of any kind be considered as equally ponderous in every equal part, and as divided into indefinitely small parts, suspended by lines, drawn from their centers, perpendicular to any horizontal plane; it is manifest that, if every part be multiplied respectively into its perpendicular line, the sum of the products will be equal to the product of the whole surface multiplied into the perpendicular distance of its center of gravity from the said plane: and that this equality of the products will subsist even if the said lines be perpendicular to any plane, though not parallel to the horizon.

of the surface which is exposed to the pressure we are concerned with, since it has been already shewn, that the pressure upon the whole is equivalent to the weight of a body of water which is equal in magnitude to the sum of all the products, made by multiplying every little part by its distance from the upper plane of the water; and this sum of products, by the statical theorem I have been mentioning, is exactly equal to the product of the whole surface or number of surfaces multiplied into the distance of the center of gravity from the upper plane of the water; it will follow, that the same product is the measure of a magnitude of water whose weight is equivalent to the pressure required. The same rule may be demonstrated by several other methods, but I have pitched upon this as the fittest for my purpose.

Another thing which *Stevinus* proposes to himself, is to determine the center of pressure upon any plane. Before we can discourse any farther about this we must declare what is meant by that center. It is then the point to which if the total pressure were applied, its effect upon the plane would be the same as when it was distributed unequally over the whole after the manner before described; or we may say it is that point in which the whole pressure may be conceived to be united; or it is that point to which if a force were applied, equal to the total pressure but with a contrary direction, it would exactly ballance or restrain the effect of the pressure. Thus if *abcd* in FIG. 23, be a vessel of water and the side *ac* be pressed upon with a force equivalent to twenty pounds of water, this force we have seen is unequally distributed over *ac*; for the parts near *a* being at a lesser depth, are less pressed upon than the parts near *c* which are at a greater depth, and therefore the efforts of all the particular pressures are united

in some point z , which is nearer to c than to a , and that point z is what may be called the center of pressure: if to that point a force equivalent to twenty pound weight be applied, it will affect the plane ac in the same manner as before by the pressure of the water distributed unequally over the whole. And if to the same point we apply the same force with a contrary direction to that of the pressure of the water, the force and pressure will ballance each other, and by contrary endeavours destroy each others effects. Suppose at z a cord zpw were fixed, which passing over the pulley p , has a weight w of twenty pounds annexed to it, and that the part of the cord zp were perpendicular to ac ; the effort of the weight w is equal, and its direction contrary to that of the pressure of the water. Now if z be the center of pressure these two powers will be in equilibrio, and mutually defeat each others endeavours.

It may be worth while to be acquainted with a rule for finding that center in all cases. We cannot have much help from *Stevinus* in this business; he undertakes only a few particulars and those which are the easiest, supposing that his reader will apply the like method to other circumstances; but they who shall endeavour to make such an application, will in most cases find it more difficult than they might possibly expect. I have for that reason devised this general rule which follows.

If any plane which happens to be proposed be produced till it intersects the upper surface of the water produced, if need be, and the line which is the common section of the two planes, be made an axis of suspension; the center of oscillation or percussion of the plane, as it is supposed to revolve about that axis, will be the center of pressure required. Thus if ac in FIG. 24, represents the plane proposed, let it be produced till it cuts the plane

$g b$ in d , now if d be made the axis of suspension of the plane ac , the center of percussion of the plane ac revolving about d , will be also the center of pressure upon the same plane.

For if the percussive forces of every point of ac be as the pressures exercised upon those points, then the center of percussion must needs be the same with the center of pressure; and that the force of percussion is every where as the pressure of the water may thus be proved. The percussive force of any point, suppose b , is as the velocity of that point, and the velocity is as the distance bd of the point from the axis of motion; so the percussive force of a is as ad , of c as cd ; since then the percussive forces of a, b, c are as the lines da, db, dc , and these lines are as the lines ea, fb, gc , perpendicular to the surface of the water, and these last lines are as the pressures upon a, b and c , it follows that the percussive forces, taking the intersection d for the axis of suspension or motion, are respectively as the pressures upon the same points; therefore the center of percussion or oscillation is the same with the center of pressure.

The geometers of the last age have prosecuted the problem of finding the center of oscillation very diligently, being excited thereto chiefly by the noble invention of pendulum clocks; the rules they have laid down for that purpose are easy enough, and the applications they have actually made of those rules are not a few. Having therefore shewn how the center of oscillation may be made use of for determining the center of pressure, I presume I have by this time sufficiently cleared up what I proposed; but for further illustration I will add a couple of examples.

Let it be required to find the pressure which a diver sustains when the center of gravity of the surface of his body is 32 feet under water. The surface of a middle sized human body is about 10 square

feet. Multiply then 32, the depth of the center under water, by 10 the surface of the body, and the product, or 32 times 10 solid feet, will be a magnitude of water whose weight is equivalent to the pressure which the diver sustains, by the rule before laid down. A cubick foot of water has been found by experiment to weigh 1000 averdupois ounces, therefore 32 times 10 feet, or 16 times 20 feet of water, will weigh 16 times 20000 averdupois ounces or 20000 averdupois pounds. This therefore is the pressure of the water to which a diver at 32 feet depth is exposed.

Again in FIG. 25, let the right angled parallelogram $abcd$ be a wall, dam, or pen of timber perpendicular to the horizon, made to keep in a pond of water, whose upper surface reaches to ab ; let ab be 20 feet, and ac 12. Let k be the center of gravity of the plane; the depth of that center k will be equal to half gb or half ac , that is 6 feet. The area of the plane is found by multiplying ac by ab or 12 by 20, it is therefore 240 square feet; multiply, according to the rule, the area 240 by gk which is 6, and the product will be 1440 cubick feet of water, which weighs so many thousand ounces, that is 90000 pounds; and that is the pressure which the dam $abcd$ sustains.

To find the center of that pressure we must make the line ab , which is the common section of the dam and the upper surface of the water, the axis of suspension of the plane $abcd$; now it appears by the discovery of *Huygens*, *Wallis* and other geometers that z , the center of oscillation of this plane so suspended, will be in the line gb which bisects this plane and is parallel to ac or bd ; and that the line gz will be two thirds of gb , that is 8 feet; and the same point z so determined is, as was proved before, the center of pressure required.

LECTURE IV.

Of the sinking and floating of bodies immersed in fluids, their relative gravities and levities, their situations and positions : the phænomena of glass bubbles accounted for.

WE are now to make our enquiries concerning the sinking and floating of bodies immersed in fluids ; their relative gravities, their levities, their situations and positions. This is the subject of *Archimedes's* two books *de Insidentibus Humido*, of which the Latin translation is yet extant, though the original in Greek be lost. I will therefore give you the substance of his doctrine with some additions. But because the last propositions of his first book demonstrate to us the postures in which a floating portion of a sphere will compose itself, and the whole second book except the first proposition of it, is entirely taken up in determining the like for the parabolick conoeid (which is a solid formed by the revolution of a parabola about its axis) I will content myself to make out and demonstrate to you the foundation upon which those theorems of his are grounded, passing by the application of it to particular solids, as being a matter that belongs more properly to geometry than to hydrostaticks.

We have already seen that fluids press upon bodies to which they are contiguous every way, and on all sides, but the pressure upon each part is not the same ; the altitude of the fluid is every where the measure of its force ; and the several parts of the same body being at different depths, must needs be differently affected. We ought therefore to consider which of all these impressions will prevail. Now it is evident that the lateral pressures do all ballance each other, being

equal, as arising from equal altitudes of the fluid, and opposite in their directions ; so that from these the body is no ways determined to any motion. But those parts of the fluid which are contiguous to the under surface have a greater altitude, and therefore a greater force than the others which are contiguous to the upper ; therefore the body must of necessity be more violently elevated by the former than depressed by the latter, and would therefore ascend by the excess of force were it devoid of gravity. Now it is easy to understand, that this excess of force is equivalent to the weight of so much of the fluid as is equal in magnitude to the bulk of the body, being the difference in weight of two columns of the fluid, whereof one reaches to the upper, the other to the under surface of the body.—

It has been objected by some against the gravitation of fluids *in proprio loco*, that bodies immersed would of consequence be violently detrued to the bottom, whereas we see in fact that the contrary is true ; some which are specifically lighter than the fluid being even buoyed up by it. Those who make this objection ought at the same time to have considered, that as the upper parts of the body are depressed, so are the under more powerfully elevated, and therefore that setting aside the consideration of the body's own weight, it ought always to ascend ; but taking in that consideration let us now see what the event will be ; and this may be easily determined. For since all bodies endeavour to descend by the force of their own weight, and to ascend by the weight of an equal bulk of the fluid in which they are immersed, it must of necessity come to pass, that if the weight of the body be greater than the weight of an equal bulk of the fluid, it will descend with a force that is equal to the difference of those two weights ; if the weight of the body be less than the weight of an equal bulk

bulk of the fluid, the weight of the fluid must prevail, and carry it upwards with a force that is equal also to the difference of the two weights; and this is the cause of the descent or ascent of bodies, as they are specifically heavier or lighter than the fluid in which they are immersed.

The same thing is often made out by another method which is somewhat different from this which we have made use of. We are to suppose, in what place soever within the fluid the body is conceived to be, that there passes by it an imaginary plane touching its under surface and parallel to the horizon. Now it has been made manifest that this fluid cannot compose itself and be at rest till every equal part of this plane sustains an equal pressure; if then the body be of an equal gravity with so much of the fluid as is equal to it in bulk, and whose place it takes up, the part of the imaginary plane, which is directly under the body will be equally affected by the pressure of the fluid and body together, which are superior to it, with the other equal parts of the same plane, which are pressed upon by the fluid alone; therefore there can be no reason assigned why the body should give way either by ascending or descending, but it ought to maintain the place given it. If the body be heavier than so much of the fluid as is equal to it in bulk, this part of the imaginary plane which is directly under it, will be pressed with a greater weight than the other equal parts of the same plane, by the excess of the body's weight, above the weight of an equal bulk of the fluid; that part must therefore yield, and the body must descend with a force equal to that excess. By a like way of reasoning we may collect, that if the body be lighter than an equal bulk of the fluid, it will be buoyed up by that part of the plane which is under it, with a force equivalent to the difference in weight of that equal bulk of the fluid and the bo-

dy; and upon the same account it must continue to ascend till every part of this imaginary plain, which is conceived to follow it, always touching its under surface, be equally pressed upon; that is, till the body be so far extant above the surface of the fluid, that the weight of a part of the fluid equal in bulk to the part of the body immersed, be equal in gravity to the whole body. And then also the part immersed will be to the whole, as the specifick gravity of the body is to the specifick gravity of the fluid. (a)

(a) The author used to confirm these conclusions by the following experiments. To try the force of descent of a solid, he used a small glass viol stopt up, having shot enough in it to cause it to descend in water, and a horse-hair tied about the neck of it, by which he suspended it to the scale or beam of a ballance.

To find the weight of a bulk of water equal to this bottle, considered as the solid, he first put it into a narrow cylindrical glass jar, and poured in water enough to cover the bottle; then taking it out, he weighed the jar and water contained; then he placed the jar upon a table, and having immersed the bottle in it, he placed a slip of wet paper upon the outside of the jar, so that the edge of the paper might appear to coincide with the surface of the water, to his eye placed in that surface produced; then taking out the bottle he poured water into the jar, till its surface rose up to the edge of the paper as before. This water then was equal in bulk to the bottle, and its weight he found by replacing the jar in the scale, and adding a separate weight to the former, sufficient to counterpoise the additional water.

Place this counterpoise in one scale and the bottle in the other, and the weight added to make an equilibrium, will be the excess of the bottle's weight above the weight of a bulk of water equal to it.

Then suspending the bottle in water, by the hair, to the arm or scale of the ballance, from the opposite scale take out the weight of the equal bulk of water, and the remaining excess, found above, will just ballance the force of the bottle's descent, by keeping the scales in equilibrio.

The force of ascent of a thin glass bubble, or any solid lighter than the fluid, may be tried by an inverted ballance, placed at the bottom of a large vessel full of water as represented in FIG. 26.

For having found the weight of a quantity of water equal in bulk to the bubble (by immersing it wholly in the water of the cylindrical glass abovementioned) and also the excess of this weight

If two fluids which will not easily mix with each other be contained in the same vessel, so that the lighter may float upon the heavier, a solid body, which is heavier then the lighter of the two fluids, and lighter than the heavier, will not suffer itself to be totally immerfed in either of them. If it be placed wholly within the heavier it will ascend, if it be placed wholly within the lighter it will descend for the reasons before given; and will never rest in any place till it be so disposed, partly within one and partly within the other, that the weight of so much of both fluids, whose place it possesses, be equal to the weight of it.

If any one will from hence compute what proportion the parts contained within each fluid bear to each other, or in what proportion the common surface of both fluids divides the whole solid, he will find, that the part contained within the heavier, must be to the part contained within the lighter, as the difference in weight of the solid and an equal bulk of the lighter, is to the difference in weight of the solid and an equal bulk of the heavier: and that the part immerfed in the heavier, is to the whole, as the difference in weight of the solid and an equal bulk of

above that of the bubble itself, as before; hang the solid by an horse-hair-loop to an arm of the inverted ballance, and connect the opposite arm to that of a common ballance by another horse-hair; then that excess of weight, placed in the opposite scale, will ballance the force of the bubble's ascent, by keeping the scales in equilibrio.

To shew that a quantity of a fluid equal in bulk to the part immerfed of a floating solid, is equal in weight to that of the whole solid, weigh a larger glass jar partly filled with water, and having taken it from the scale, let a smaller jar float upon the water and mark its altitude with a wet paper, as before; then having taken out the lesser jar and filled the larger with water up to the mark, replace the larger in the scale, and the lesser jar being put to the former weights in the opposite scale, will produce an equilibrium.

the lighter, is to the difference in weight of an equal bulk of the heavier and the same equal bulk of the lighter. (*b*)

Hence if we would be so scrupulously exact we may easily correct that small error in the rule of *Archimedes* which was before delivered for floating bodies; namely, that the part immersed was always to the whole, as the gravity of the solid is to the gravity of the fluid. For since the air is an heavy fluid, though it be the least heavy of all others, yet by resting upon the upper surface it has this effect, that in reality it will not permit a solid to be altogether so deeply immersed as it would otherwise be if the air were removed, which the rule supposes. Allowing then for the air's presence we may thus express the proportion: That the part immersed is to the whole, as the difference in weight of the solid and an equal bulk of air, is to the difference in weight of an equal bulk of the fluid and the same equal bulk of air. Whoever will compare these two rules together, will find that their difference is altogether inconsiderable: we may therefore still very securely make use of the old one without any further scruple.

The sum of what has been said comes to this, that if a solid be heavier bulk for bulk than the fluid in which it is immersed, it will sink till it arrives at the bottom, and the force of its descent will be equi-

(*b*) Let the parts of the solid contained within the heavier and the lighter fluid be *A* and *B*, in FIG. 27, and the specifick gravities of the respective fluids as *a* and *b*; then since the absolute gravity or weight of any body is compounded of its magnitude and specifick gravity, the weight of a quantity of the heavier fluid equal in magnitude to the part *A*, is aA , and the weight of a quantity of the lighter fluid, equal in magnitude to the part *B*, is bB , and the sum of their weights is $aA + bB = c \times A + B$ supposing *c* is as the specifick gravity of the solid $A + B$. Hence $aA - cA = cB - bB$, and consequently $A : B :: c - b : a - c$; and conjointly $A : A + B :: c - b : a - b$.

valent

valent to the difference of its weight and the weight of an equal magnitude of the fluid. If it be lighter than the fluid in which it is immerfed, it will constantly ascend till it be fo far extant above the furface of the fluid, that its whole weight be equal to the weight of that part of the fluid whose place it takes up ; and the force with which it ascends will be equivalent to the difference of its weight and the weight of an equal magnitude of the fluid. If it be immerfed in either of two fluids differently heavy; which are contiguous, and it be of a middle gravity between both the fluids, it will move towards their common furface, and reft in fuch a pofition that the parts of both fluids whose place it takes up have an equal gravity with the folid itfelf. If a body be of equal gravity with the fluid in which it is immerfed, it will retain any pofition which is given it : and this is the reafon why a bucket in a well is without difficulty drawn up as long as it is under water, and that we perceive not its weight till it begins to get above the furface : by the fame reafon a bucket full of wax, which is nearly of the fame gravity with water, would not be difficult to draw up whilft under water ; now they cannot eafily answer that wax is in its own element, and does not therefore gravitate.

All bodies do actually retain their whole gravity when immerfed in a fluid, but that is rendered ineffectual by the contrary preffure of the fluid, fometimes in part and fometimes altogether, according as that gravity of theirs is greater, equal, or lefs than the gravity of an equal bulk of the fluid, which is the meafure of the force which refifts their defcent. We may fay that the gravity of bodies within fluids is of two forts, whereof one is abfolute and true gravity, the other apparent and relative. The abfolute gravity is the whole force with which the
body

body tends downwards, the relative and vulgar is only the excess of gravity by which a body has a greater tendency downwards than the ambient fluid. The parts of fluids and all other bodies do gravitate *in proprio loco*, taking gravity in the first sense; according to the latter sense and acceptation of gravity bodies do not gravitate *in proprio loco*, that is, being compared together they do not preponderate, but hindering each others endeavours to descend they keep their places as if they were devoid of gravity. Thus in water, bodies which by their gravity, greater or lesser, do ascend or descend, may relatively and apparently be said to gravitate or levitate, and their relative gravity or levity is the excess or defect by which their true gravity does either exceed the gravity of water, or fall short of it. But if they neither descend by preponderating, nor ascend by yielding to the preponderating water, though they do by their real and absolute weights increase the weight of the whole, yet relatively and in the sense of the vulgar they do not gravitate in the water.

We may also in this place take notice of an objection which is sometimes made use of against the gravitation of fluids *in proprio loco*. They tell us, if fluids gravitate *in proprio loco*, that then a body as it happens to be immersed at different depths would have a different weight, according as it is pressed upon by different altitudes of the same fluid, which does not appear in fact. We may answer that it has and ought to have the same relative weight, though it be pressed upon by different altitudes at different depths. For its absolute weight does every where continue the same, and the relative weight is the excess of that absolute weight above an equal bulk of the fluid. Therefore if the weight of that equal bulk of the fluid be at all depths the same, as it certainly is in fluids which are not compressible, that excess
and

and therefore the relative weight, will every where be the same also.

If the fluid be compressible, as air is, and the lower parts be condensed by the weight of the upper, then indeed the relative weight of a body in the air at the bottom of a valley, will be less than its relative weight at the top of a mountain; an equal bulk of air weighing more in the valley than upon the mountain, and consequently taking more from the real and absolute weight of the body in the former case than in the latter. For the same reason if a body be weighed in fresh water, and sea water, its weight will be less in the latter than the former, because the excess of its real and absolute weight above the weight of an equal bulk of sea water, is less than the like excess above an equal bulk of fresh water; sea water being about a thirtieth or fortieth part heavier than fresh. Upon this account also if two bodies of different specifick gravities be equiponderant and consequently of different magnitudes, suppose the one be of copper and the other of lead, and the bulk of the copper be greater than the bulk of the lead, as it must be to be of the same weight, putting the two metals in opposite scales of a ballance, we shall find them to rest in equilibrio; but if we place the ballance under water, they will be no longer in equilibrio, and the lead will preponderate. For the absolute weight of each being diminished by the weight of a bulk of water equal to itself, the weight of the copper will be more diminished than the weight of the lead. Thus if two bodies of different specifick gravities be brought to a most perfect equilibrium when the air is at lightest, they will no longer remain so when the air changes and becomes heavier; and this is the foundation of the statical baroscope described by Mr. *Boyle* in the *Philosophical Transactions*.

The phænomena of glass bubbles and images, which are fitted several ways to ascend and descend in fluids, have been very much celebrated by the philosophers of the last age. The whole mystery depends upon this, that by a greater or lesser pressure on the bladder at the end of the vessel, FIG. 28, or by heat and cold, there is an alteration made of their weight, and by this alteration of their weight they become sometimes heavier sometimes lighter than that part of the fluid whose place they possess, and do therefore, for the reasons which have been lately mentioned, sometimes ascend sometimes descend with a pleasing variety. These bubbles consist usually of three different materials; of glass which is heavier *in specie* than the fluid, of air which is lighter *in specie* than the fluid, and of the fluid itself. As long then as that aggregate of bodies is lighter than an equal bulk of the fluid, it will float, but if it grows heavier than so much of the fluid, it must necessarily sink. Now when there is any competent pressure, whether produced by weight or otherwise, upon the water in which the bubble is commonly immersed, because the glass is a firm body and the water though a fluid suffers no compression, the air included in the bubble, being a springy and very compressible body, will be compelled to shrink, and thereby possessing less room than it did before, the contiguous water will enter the neck of the bubble and succeed in its place; which being a body about 850 times heavier than air, the bubble will thereby become heavier than an equal bulk of water, and will consequently descend; but if the force or pressure be removed, the imprisoned air will by its own spring free itself from the intruding water, and the aggregate of bodies that make up the bubble, being thereby grown lighter than an equal bulk of water, will again ascend. The dilatation and contraction of the included air is therefore

fore the cause of these changes ; if then that dilatation and contraction be any other way procured than by pressure (as it may proceed from heat and cold) the event will be the same. And this may suffice to account for the phænomena of bubbles.

I promised in the beginning of this discourse to give you the foundation of those propositions of *Archimedes*, in which he demonstrates the posture of floating bodies ; it may thus be expressed ; that all floating bodies affect such a posture, that the center of gravity of the part immersed, be situated perpendicularly under the center of gravity of the part extant : the body will otherwise never rest and cease to fluctuate. For if the floating body be imagined to be divided into two parts by the surface of the fluid, it will be easy to conceive, that the part immersed endeavours to ascend, and the extant part to descend with equal forces ; otherwise the body would be either more or less immersed. Now the part immersed endeavours to ascend by the perpendicular passing through its center of gravity, and the part extant to descend by the perpendicular passing through its center of gravity ; therefore unless those perpendiculars do coincide, or which is the same thing, unless the center of the part immersed be situated perpendicularly under the center of the part extant, there will be no hindrance of those endeavours, but a motion will be produced, and for the same reason continued till that posture be obtained ; and in that posture the body will acquiesce, the endeavours being then equal and directly contrary to each other, and thereby restraining each other.

LECTURE V.

The hydrostatical ballance explained, with the methods of determining the specifick gravities of all sorts of bodies thereby.

IT having been proved that bodies ascend or descend in fluids with a force that is equal to the difference in weight of the body immersed and an equal bulk or magnitude of the fluid itself, we are hence furnished with a very accurate and easy way of finding out the specifick gravities of all manner of bodies whether fluid or consistent, and of comparing them together. Bodies are said to be specifically or *in specie* heavier or lighter one than another, when being equal as to magnitude, the weight of the one does exceed or fall short of the weight of the other. Thus the specifick gravity of quicksilver is about 14 times greater than that of water; for if you take an equal quantity of each as to magnitude, suppose a pint, the the pint of quicksilver will weigh about 14 times as much as the pint of water.

Several methods have been proposed and more may be still invented to determine in what proportion bodies differ from one another as to their specifick gravities; yet after all, most men have with good reason preferred the use of the hydrostatical ballance for exactness and convenience. It is very probable that *Archimedes* was the first that ever attempted this business with any success, in order to discover the cheat of the workman that had debased king *Hiero's* crown, and though the way he then made use of, be certainly much inferior to that we have been speaking of by the hydrostatical ballance (as may be perceived by the account which *Vitruvius* gives of it) yet so pleased was he to gain his end by any means, that upon this occasion not being able to contain his joy,

joy, like a madman leaping from the bath, naked as he was, he is said to have ran about the streets of *Syracuse*, crying out his *Εὐρηκα* wherever he came. I will not here stay to enumerate and explain those various methods that have been thought of for finding out the specifick weight of bodies, but will confine myself to the business I have undertaken, and shew what helps we have from hydrostaticks, and how suitable they are to our present purpose.

First then if it be required to find out what proportion the specifick gravity of a fluid and solid body have to one another, and the solid be heavier than the fluid so that it may sink when immersed in the fluid, we are to weigh the solid both in air and in the fluid. Now it has been proved before, that its weight in the fluid will be less than its weight in the air, by the weight of so much of the fluid as is equal in bulk to the solid; but the specifick gravity of the fluid, is to the specifick gravity of the solid, as the absolute weight of an equal bulk of the fluid, is to the absolute weight of an equal bulk of the solid; therefore the specifick gravity of the fluid, is to the specifick gravity of the solid, as the difference in weights of the solid in air and in the fluid, is to the weight of the solid in the air. If the fluid be common clear water and its specifick gravity be expressed by an unit, as is usual and very convenient upon several accounts, then to find a number which will express the specifick gravity of the solid, we must divide the weight of the solid in air by the difference of the weights of the same in air and in water, the quotient will be the number required.

An example will clear the whole matter. Suppose that a piece of copper weighed in air comes to 45 grains, and when weighed in water but to 40 grains; the difference of these two weights, which is 5 grains, is equal to the weight of so much water as is equal in bulk to the piece of copper. Therefore the specifick gravity of water, is to the specifick gravity
E of

of copper, as 5 to 45. The specifick gravity of water is here expressed by the number 5, if instead of that it were to be expressed by an unit, we must divide 45, the weight of the copper in air, by 5 the difference of 45 and 40 its weight in air and water, and the quotient, which is 9, will express the specifick gravity of the copper as an unit does that of water, 9 bearing the same proportion to 1 as 45 did to 5.

If the solid body to be examined be specifically lighter than the fluid, we would compare it with, so that it cannot sink by its own weight, but is continually buoyed up by the heavier fluid, we may by a compound ballance find out its relative levity in the fluid, or the force with which it endeavours to ascend.

It was yesterday shewn by an experiment made with such a compound ballance, that the force of ascent is equal to the difference of weights of the ascending body and an equal bulk of the fluid which invirons it; therefore the weight of so much of the fluid as is equal in bulk to the body, is the sum of two weights, whereof one is the absolute weight of the body in air, and the other is equal to the force of ascent, being the weight which is applied to the compound ballance to reduce the ascending body to an *equilibrium*. Hence it follows that the specifick gravity of the fluid, is to the specifick gravity of the solid, as that sum of the two weights, is to the absolute weight of the solid. If the fluid be common water, and its specifick gravity be expressed by an unit, we must divide the absolute weight of the solid by that sum of weights, and the quotient will express the specifick gravity required.

To illustrate this by an instance, let us suppose that a piece of dry elm weighs in air 36 grains; this wood being lighter than water, will not of it self sink in it; let then a weight be applied to the superior beam of the compound ballance to detain it under water and to keep it *in equilibrio*, and the weight necessary for that

that purpose be found to be 24 grains; this weight of 24 grains being as we have already proved, equal to the difference of the weight of the elm and an equal bulk of water; if it be added to the lesser weight of the elm, which was 36 grains, the sum which is 60 grains will be the weight of that equal bulk of water. Therefore the specifick gravity of water is to that of elm as 60 is to 36; if instead of 60 which does now express the specifick gravity of water, you would rather make use of an unit for that purpose, we must divide 36, which is the weight of the elm in air, by 60 the sum which was before mentioned, the quotient 0,6 will express the specifick gravity of elm, as an unit does the specifick gravity of water; and it is evident, that 0,6 has the same proportion to an unit that 36 had to 60.

I know not whether this way of examining bodies which are lighter than the fluid they are compared with has ever yet been put in practice, there seeming to be too great a difficulty in making the experiment, which yet may be much lessened, if not taken away, by a well contrived instrument; however it is certain that the calculation is much more easy in this method than in that other which I will now describe.

To the body which we would examine which is lighter than the fluid with which it is to be compared, we must annex another body (by tying them together with an horse-hair or otherwise) which is specifically heavier than the fluid; so that both taken together as one compound body, may be likewise specifically heavier than the fluid and sink in it; weighing then the heavier body singly, and also the compound, both in air and in the fluid, we must thus make our calculation. Subtracting the weight of the heavier body alone in the fluid, from its weight in air, what remains will be the weight of so much of the fluid as is equal in bulk to the heavier body; again subtracting the weight of the compound body

in the fluid, from its weight in air, what remains will be the weight of so much of the fluid as is equal to the compound body in bulk; taking then the former difference from the latter, that is, taking the weight of so much of the fluid as is equal in bulk to the heavier body, from the weight of so much of the fluid as is equal to the compound body, or heavier and lighter together, what remains will be the weight of so much of the fluid as is equal in bulk to the lighter body; and the proportion which this weight bears to the weight of the lighter body in air, will be the proportion of the specifick gravity of the fluid to the specifick gravity of the lighter body. If the number which expresses the specifick gravity of the lighter body be divided by the number which expresses the specifick gravity of the fluid, the quotient will also express the specifick gravity of the lighter, whilst an unit expresses that of the fluid.

Thus if a piece of elm weighs in air 15 grains, having fixed to it a piece of copper that the compound may sink in water, let us suppose that the copper alone in air weighs 18 grains, in water 16 grains, the aggregate of the copper and elm in air will be 33 grains; suppose again we find by making trial of it, that the aggregate in water comes to 6 grains; if we subtract 16 the weight of the copper alone in water, from 18 its weight in air, the 2 grains which remain will be the weight of a bulk of water equal to the copper; also if we subtract 6 the weight of the compound in water, from 33 the weight of it in air, the 27 grains which remain will be the weight of a bulk of water equal to the compound. Taking then 2 the weight of water equal in bulk to the copper, from 27 the weight of water equal in bulk to the copper and elm together, the 25 grains which remain will be the weight of the water equal in bulk to the elm. The weight of the elm itself in air was 15 grains; therefore water is to elm in specifick gravity as 25 to 15;
now

now as 25 is to 15 so is 1 to 0, 6 as any one may find by the rule of three. The third term in the proportion being in our case an unit, we are only to divide the second by the first, or the weight of the solid in the air by the weight of an equal bulk of water, and the quotient will be the fourth term in the proportion, expressing the specifick gravity of the solid, as an unit expresses that of water.

Having by this time I hope sufficiently explained the method of comparing solids and fluids together as to their gravities, I am now to shew how solids are to be compared with solids, and fluids with fluids. This will require but few words being easy and obvious enough. Solids may be compared with solids by the mediation of a fluid; and fluids may be compared with fluids by the mediation of a solid; sometimes indeed it may happen that a fluid may be weighed as a solid body within another fluid with which it will not mix, by placing it in the glass bucket; thus may quicksilver most conveniently be compared with water (*a*). Suppose it were required to determine what proportion the gravity of copper has to the gravity of elm; these two cannot immediately be compared together hydrostatically, but we may as has been already shewed compare each of them with water, and then we may conclude that the specifick gravity of copper, is to the specifick gravity of elm, in a proportion, which is compounded of that of the specifick gravity of copper to water, and that of the specifick gravity of water to elm. If copper be to water

(*a*) FIG. 29. represents the hydrostatical ballance, whereby solid bodies may be weighed in the glass bucket *a*, first in air and then in water. In the latter case the slit *b* in the circular plate, must first be slipt upon the neck *c*, and rest upon the square shoulder underneath; that the weight of the plate, being equal to that of a quantity of water equal in bulk to the empty bucket, may restore the equilibrium of the empty scales. The specifick gravities of fluids are determinable by the glass ball *d*, described in the sequel of the lecture.

as 9 to 1, and water be to elm as 1 to 0,6, copper will be to elm as 9 to 0,6 or as 90 to 6, or as 15 to 1.

Suppose again two fluids were proposed to be compared together; let one of them be a parcel of oyl of vitriol bought in the shops which you suspect to be not the best, and you would examine whether its gravity be to the gravity of water as 17 to 10, as it ought to be if good; comparing it with glass by the method before described, you find its gravity to that of glass to be as 7 to 15; comparing the same glass with water you find the gravity of glass to that of water as 3 to 1; hence by compounding the proportions of 7 to 15 and 3 to 1, you know that the gravity of your oyl of vitriol is to the gravity of water as 7 to 5, or as 14 to 10, whereas it ought to have been as 17 to 10. This way of comparing fluids together is universal, and may be practised with a ballance of any form. The fabrick of our instrument does indeed in this particular somewhat shorten the operation, and therefore it will not be amiss to shew how we make our calculation from it.

The glass ball you may remember was heavier than an equal bulk of water, as was evident by its sinking in it, and by an experiment purposely made, its weight was found to be to the weight of an equal bulk of water, as $18\frac{1}{3}$ to 10; oyl of vitriol, which is one of the heaviest fluids excepting quicksilver, is to water as 17 to 10, therefore the ball may be used for examining any liquor that is less heavy than oyl of vitriol, since it will sink even in that oyl. The excess of weight of the ball above that of an equal bulk of water, was counterpoised by an equal excess of weight, of the opposite scale of the ballance, above that of the scale to which the ball was fixed; and by that means it was sustained in the water *in equilibrio*. We may conceive the ball so ballanced in the water, as if it were a parcel of the water congealed into that shape; and therefore if we substitute for water in the vessel

vessel some other liquor of a different gravity, this *equilibrium* will be no longer preserved; we are therefore continually to put weights into the ascending scale, till we have again reduced the ballance to the same state; and the weight we have put into either scale, will be the difference in gravity of two bulks, one of water and the other of the liquor to be examined, which are equal to one another, and each equal to the bulk of the ball. This bulk of water has been found to be 803 grains; if therefore we add to 803 the number of grains which were put into the scale to which the ball is annexed, or subtract from 803 the number of grains which were put into the opposite scale, the result will be the weight of a bulk of the liquor under examination equal to the ball; and the specifick gravity of water will always be to the specifick gravity of the other liquor, as 803 to the resulting number. If we divide the result by 803 the quotient will express the gravity of the other liquor as an unit expresses that of water.

To illustrate this by an example, let it be proposed to find the gravity of milk; immersing the ball as it is fixed to the ballance in that liquor, we find it necessary to put 28 grains into the scale to which the ball hangs, in order to reduce the beam to its horizontal situation; adding then 28 to 803 the sum will be 831, and the specifick gravity of water to that of milk, will be as 803 to 831.

Thus then may all bodies of what kind soever be compared together as to their intensive weights; I might have added other methods which are also hydrostatical and fitted to the same purpose, but those already described are sufficient and seem indeed to be the most convenient. However I will here mention one other way of examining the gravity of fluids which is of very good use upon some occasions. The foundation of it is this, that if a body be made successively to float upon two fluids of different gravities, the spe-

cifick weight of the lighter will be to the specifick weight of the heavier, as the magnitude of the part of the floating body which is immerfed in the heavier, is to the magnitude of the part immerfed in the lighter. For, the bulks or volumes of both fluids that are equal to the parts refpectively immerfed in them, having the fame absolute weight with the whole floating body, as hath been proved before, will be of equal absolute weights, and confequently their specifick gravities will be reciprocally as their magnitudes, or which is the fame thing, reciprocally as the magnitudes of the parts immerfed. If therefore a body of a regular figure could be provided fo that the part of it which is immerfed, might always be accurately and eafily meafured, this way would be expeditious enough. A truly cylindrical glafs vefel feems to be the fitteft for this purpofe; for the part immerfed will be always as its depth. The gravity therefore of the fluid may readily be eftimated by a proper fcale of parts in arithmetical progreflion applied to the fide of the cylinder, or more readily by infpection only of another fcale which might be mufically divided by ways which I will not ftay here to describe. For, any one that fhall attempt to get fuch a cylinder, as will be convenient for his purpofe, of an exact figure and truly poifed, that it may always ftand erect, will perhaps find it more difficult to obtain than at firft he expected.

Nevertheless upon fome occafions this method may be of fingular ufe. We may examine by this means whether a liquor propofed be genuine as to its gravity, though we do not learn from hence what is the precise quantity of that gravity. The thing is common among chymifts; they make ufe of an hollow ball of glafs having a flender ftem or pipe annexed to it, which is fo poifed that when the ball is immerfed in any liquor, the ftem may ftand erect and be in part extant above the furface of it. This
glafs

glafs they place in feveral liquors which they know by other means to be good in their kind, and put marks upon the ſtem, which ſhew the different degrees of immerſion in the different liquors. Then if any liquor of the ſame name be afterwards to be examined, ſuppoſe it were oyl of tartar *per deliquium*; if the glaſs ſink lower in it than the mark they had formerly made for that oyl, they conclude it has not its juſt gravity, and is probably adulterated with water; but if it ſink not ſo low as the mark for alcohol of wines, they conclude that the alcohol is too heavy, and therefore not ſufficiently rectified (a). This inſtrument by a ſmall alteration may be fitted to examine whether ſolids have the true ſtandard weight of their kind, as any one may perceive by the deſcription of Mr. Boyle's eſſay-inſtrument, publiſhed in the Philoſophical Tranſactions.

After all that has been hitherto ſaid, there may yet remain ſome few difficulties to be removed, and ſome cautions to be given. It may happen that the body to be examined may conſiſt of ſmall fragments, or may be a powder, or may imbibe the water it is weighed in, ſo as to appear heavier than it really is, or may be diſſoluble in water. If it be made up of ſmall fragments or be a powder, we muſt of neceſſity in this caſe make uſe of the glaſs bucket, which we are not obliged to do when the body to be weighed is entire, and of a conſiderable magnitude. For then we may, if we think fit, make uſe of a ballance of the uſual form, and by ſuſpending the body with an horſe-hair (which is nearly of the ſame gravity with water) to the under ſide of either of the ſcales, we may weigh it both in air and in water. Putting then a convenient quantity of the fragments or powder into the bucket; we firſt find the weight in air, afterwards we muſt warily and little by little put into the bucket, whiſt it is yet kept in air, and hath the powder

(a) See Phil. Tranſ. N^o. 384 and 413.

der or fragments in it, a convenient quantity of the same water it is to be weighed in, that the liquor may have time to insinuate itself between the dry bodies and even the corpuscles of the powder, and expel thence the air that was lodged in the intervals betwixt them; which little aerial portions, if not thus seasonably expelled, would upon the immersion of the vessel, produce in the water store of bubbles, that would buoy up or fasten themselves to the fragments or other small bodies, and make the experiment uncertain or fallacious. And if it be a powder that is to be weighed, unless it be beforehand thoroughly wetted, and thereby freed from aerial particles, and reduced to a kind of mud, there is danger that some dry corpuscles of the powder will, when the vessel is under water, be buoyed up and get out of it, and floating on the surface of the incumbent water take off from the true weight, that the immersed powder should have in that liquor.

If the body to be weighed be subject to imbibe water too readily, it may be cased with a coat of bees-wax, and then we must proceed in our calculation according to the method which was before described for determining the gravities of bodies lighter than water, or the fluid they are weighed in, by adding a more ponderous body; and as there we weighed the heavier body by itself in air and in water, and afterwards the compound of the heavier and the lighter both in air and in water, so here we must weigh the bees-wax before we apply it both in air and water, and afterwards the compound of the bees-wax and the other body both in air and in water. If the body proposed be dissoluble in water, we may weigh it in other liquors, which will not dissolve it.

Mr. *Boyle* tells us upon this occasion, considering that except quicksilver, the visible fluids we can command are either of an aqueous or of an oily nature, and that most bodies whereof we can make solutions
in

in liquors of the former, will not (at least sensibly) suffer themselves to be dissolved by those of the latter kind, whilst a proposed solid is weighing in them; he presumed that the most saline bodies, such as alum, vitriol, sal gem. borax, sublimate and others, might be commodiously weighed in oleous liquors; and amongst these upon several accounts he made choice of spirit of turpentine. If then we follow his choice, we may determine what proportion the gravity of the solid proposed has to the gravity of that spirit; and by other ways, which have been already explained, finding the proportion of the gravity of the same spirit to the gravity of water, by compounding their two proportions, we shall obtain the proportion of the solid's gravity to that of water.

Most men who treat of these matters do now a days compare all other bodies with water, whose specific gravity they express by an unit. It may perhaps be objected that we cannot discover the proportion between a solid body and water in general, but only between the proposed body and the particular water it is weighed in, because there may possibly be a great difference between liquors which are called common water. Mr. Boyle has supplied us with an answer to this objection; I will therefore give you his words.

“ Having had, says he, the opportunity as well as curiosity upon several occasions to examine the weight of divers waters, some of them taken up in places very distant from one another, I found the difference between their specific gravities far less, than almost any body would expect. And if I be not deceived by my memory the difference between waters, where one would expect a notable disparity, was but about the thousandth part (and sometimes perhaps very far less) of the weight of either. Nor did I find any considerable difference between the weight of divers waters of different kinds, as spring water, river water, rain water, and snow wa-

“ter; though this last was somewhat lighter than any
“of the rest. And having had the curiosity to procure
“some water brought in England, if I much misre-
“member not, from the river Ganges itself, which
“some travellers tell us from the press is by a fifth
“part lighter than our water, I found it very little, if
“at all, lighter than some of our common waters.”

It may also be possibly objected, that we take the weight which bodies have in the air for their absolute weight, whereas their absolute weight is what they would weigh in *vacuo*. I allow that all bodies have less weight in air than their absolute and real weights in *vacuo*; but if we consider that this diminution of real weight is in most bodies but about the thousandth part of the whole, and in metals, which are the heaviest kind of bodies, much less, this objection will cease to be considerable: yet if any one has a mind to be so needlessly accurate in a matter which will not bear that nicety upon other accounts, he may add the number which expresses the specifick gravity of air, to all the other numbers of any table of bodies, and by that means correct the whole error which rises from this source.

Though this way of examining the gravities of bodies hydrostatically be preferable to all others, yet it is not free from some uncertainties. Bodies themselves though they have justly the same name and are referred to the same kind, have not any exact common standard of weight, and some little errors will inevitably arise in physical experiments though made with the utmost accuracy. We must therefore be as cautious as we can. When we weigh any thing in water or any other liquid, great care ought to be taken, that no part of the body to be weighed touch the bottom or sides of the vessel, or rise above the surface; that no bubbles of air stick to and buoy it up; that no drops of the liquor touch or adhere to the scales or beam. Several other cautions there are which may
best

best be learned by experience. I therefore design to-morrow to practise the rules which have been now laid down, and to suggest some further uses of this sort of experiments.

A table of the specifick gravity of bodies.

Fine gold —————	1.9640	Oyl of tartar —————	1.550
Standard gold —————	1.8888	Bezoar —————	1.500
Quicksilver —————	1.4000	Honey —————	1.450
Lead —————	11.325	Gum arabick —————	1.375
Fine silver —————	11.091	Spirit of nitre —————	1.315
Standard silver —————	10.535	Aqua fortis —————	1.300
Bismuth —————	9.700	Pitch —————	1.150
Copper —————	9.000	Spirit of salt —————	1.130
Cast brass —————	8.000	Craffamen. of hum. blood	1.126
Steel —————	7.850	Spirit of urine —————	1.120
Iron —————	7.645	Human blood —————	1.054
Tin —————	7.320	Amber —————	1.040
Glass of antimony —————	5.280	Serum of human blood	1.030
A pseudo-topaz —————	4.270	Milk —————	1.030
A diamond —————	3.400	Urine —————	1.030
Clear crystal glass —————	3.150	Dry box-wood —————	1.030
Island crystal —————	2.720	Sea water —————	1.030
Fine marble —————	2.700	Common water —————	1.000
Rock crystal —————	2.650	Camphire —————	0.996
Common green glass —————	2.620	Bees wax —————	0.955
Stone of a mean gravity	2.500	Linseed oyl —————	0.932
Sal gemmæ —————	2.143	Dry oak —————	0.925
Brick —————	2.000	Oyl olive —————	0.913
Nitre —————	1.900	Spirit of turpentine —————	0.874
Alabaster —————	1.875	Rectified spirit of wine	0.866
Dry ivory —————	1.825	Dry ash —————	0.800
Brimstone —————	1.800	Dry maple —————	0.755
Dantzick vitriol —————	1.715	Dry elm —————	0.600
Allum —————	1.714	Dry fir (a) —————	0.550
Borax —————	1.714	Cork —————	0.240
Calculus humanus —————	1.700	Air —————	0.001 $\frac{1}{4}$
Oyl of vitriol —————	1.700		

(a) These are the specifick gravities of dry wood. For Dr. Jurin has observed that the substance of all wood is specifically heavier than water, so as to sink in it, after the air is extracted from the pores and air-vessels of the wood, by placing it in warm water under a receiver of an air-pump; or if an air-pump cannot be had, by letting the wood continue some time in boiling water over a fire. Thus he found also, that some human *calculi*, contrary to common opinion, are almost as heavy as bricks and the softer sort of paving stones. *Phil. Transf.* No. 369.

The specifick gravities of human blood, its *craffamentum* and *serum* are taken from very accurate experiments made by the same judicious author. *Phil. Transf.* No. 361.

LECTURE VI.

The praxis of the hydrostatical ballance, the specifick gravities of several bodies actually found out, with an account of the various uses of such enquiries.

IT may now be expected, after so much of our time spent in explaining and practising the hydrostatical ballance, that I should give some account of the usefulness of such tryals. To do this in its fullest extent would take up more of our time than we can conveniently spare upon that subject alone. There have been whole books written upon this matter by *Ghetaldus* and *Mr. Boyle*, who have nevertheless left several things untouched, which might have been pertinent enough to their purpose. Any one who has in the least meddled with natural philosophy, must needs be sensible of what great importance it is to be able to compare bodies together, as to their magnitudes, densities, and the quantities of matter they contain. Now this may be done by the help of the instruments we have been describing and making use of; for the density of any body is as its specifick gravity, and the quantity of matter it contains is as its absolute weight; therefore whatever comparisons we can make of the magnitudes, specifick gravities and absolute weights of bodies, the same will hold good as to their magnitudes, densities and quantities of matter considered together. Hence did our famous *Sir Isaac Newton* conclude that water has about 40 times more pores than solid parts. Hence also he made it evident that the forces of bodies to reflect and refract light are very nearly proportional to their densities, excepting that unctuous and sulphurous bodies refract more than others of the same density.

It would be needless to heap up instances of this kind which any thinking person cannot miss of. But
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though it be impossible to declare all the uses there may be of comparing bodies thus together, I will however give some certain rules for such comparisons, and shew how from any two of these three things, magnitude, specifick gravity and absolute weight given, the other may be determined. Thus will our geometry be enlarged, and we may be able to find out the magnitude of any body however irregular, by its weight and specifick gravity; and the same way our staticks may be improved to find out the weight of any body how great soever, suppose it were a whole building, by the magnitude and specifick gravity of the materials which compose it. After this I will shew how any mixture of any two bodies may be discovered, as the allay of gold and silver, and other problems of the like nature. In the third and last place I will give some instances of the usefulness of these enquiries to physicians, chymists, apothecaries, jewellers, goldsmiths and others, who by these means may be able to judge whether the materials they deal with be rightly qualified for their purpose or not.

To determine any one of these three things, magnitude, specifick gravity and absolute weight, the other two being supposed to be given or known, we may observe the following Rules.

1. If bodies compared together be of equal magnitudes, their absolute weights will be as their specifick gravities.

2. If bodies compared together be of the same specifick gravity, their absolute weights will be as their magnitudes.

3. If bodies compared together have their absolute weights equal, their magnitudes and specifick gravities will be reciprocally as one another.

Hence if bodies compared together be neither of equal magnitudes, nor of the same specifick gravity, nor the same absolute weight, then

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4. Their absolute weights will be in a compound ratio of their specifick gravities and their magnitudes.

5. Their specifick gravities will be in a compound ratio of their absolute weights directly and magnitudes inversely.

6. Their magnitudes will be in a compound ratio of their absolute weights directly, and their specifick gravities inversely.

To fit these rules to our purpose, we ought further to know the exact weight of some certain magnitude of a determinate body whose specifick gravity we can readily compare with that of all other bodies; such I take water to be, which I therefore make choice of. Now a cubick foot of water weighs precisely 1000 averdupois ounces; I have found it to be very nearly so myself, and by comparing several experiments that have been made by others for this purpose, I find there is sometimes an excess at other times a defect, but the difference is always inconsiderable. It falls out very happily and is a great ease in calculation, that one cubick foot of water, with which other bodies are most easily compared, and whose specifick gravity is commonly expressed by an unit, should weigh such a round number of ounces.

We have another convenience by taking the weight thus by ounces, that we can by this means the more easily express our deductions by other ancient and modern weights. For it has been sufficiently proved that the ancient and modern Roman ounce has no sensible difference from our averdupois ounce, and it is well known that other ancient and modern weights are most easily referred to those of the Romans.

Our averdupois ounce contains $437\frac{1}{2}$ grains troy, and our averdupois pound contains 7000 grains troy, so that the averdupois ounce is to the troy ounce nearly as 51 to 56, and the averdupois pound to the troy pound nearly as 17 to 14. We may therefore by any
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of these proportions make a reduction from either of these weights by the other.

It being evident then by experiment that a cubick foot of water weighs 1000 averdupois ounces, we may hence shorten and facilitate the rules which were laid down in more general terms, by compounded proportions, and instead of them make use of these following; which all along suppose the specifick gravities of bodies to be expressed by a scale of numbers wherein 1000 is put for the specifick gravity of water, and their absolute weight to be expressed by the number and parts of averdupois ounces, and their magnitude to be expressed by the number and parts of a cubick foot.

1. The absolute weight of any body is equal to the product which arises by multiplying its magnitude and specifick gravity together.

2. The specifick gravity of any body is equal to the quotient of its absolute weight divided by its magnitude.

3. The magnitude of any body is equal to the quotient of its absolute weight divided by its specifick gravity.

These three rules may be sufficient and are easy enough to be practised: I will give an example fitted to each. Suppose an architect being about to build a church were desirous to know beforehand what weight of lead is requisite to cover it, in order to compute the expence he must be at. He knows by the dimensions he has proposed to himself, that the area to be covered is 30000 feet, and is satisfied by experience that the thickness of an hundredth part of a foot is sufficient; multiplying then 30000 by $\frac{1}{100}$, or dividing it by 100, the magnitude of the lead whose weight he requires will be 300 cubick feet. By making experiment or by some table he finds that the specifick gravity of lead is 11325 at the same time that the

specifick gravity of water is 1000. Multiplying therefore according to the first rule the specifick gravity 11325 by 300 the magnitude, the product will be 3397500, which is the number of ounces that the lead will weigh. There are 35840 ounces in a tun, if therefore the product be divided by the number of ounces, the quotient will be the number of tuns requisite to cover the whole building, which amounts to 94 and about $\frac{4}{5}$ of a tun.

Suppose again for an example of the second rule that a polished parallelopiped of fine marble is in magnitude equal to 4 cubick feet, and when weighed comes to 6 hundred weight and 3 pounds, and it were required to find the specifick gravity of it. An hundred weight being 112 single pounds, 6 hundred weight and 3 pounds will be equal to 675 single pounds, which multiplied by 16 makes 10800 ounces. If then according to the second rule, we divide the weight of marble by 4 its magnitude, the quotient 2700 will be the specifick gravity of marble as 1000 is the specifick gravity of water. By this method we find the specifick gravities of bodies without the help of hydrostaticks, but as this method can seldom be put in practice by reason of the irregular figures of most bodies which we may have occasion to examine, so is it never so accurate as the hydrostatical way which I have already explained.

The third rule is of excellent use for determining the magnitude of any body how irregular soever it be, provided we can assign both its absolute and specifick weight. Let several fragments of coral be proposed, whose specifick gravity we find to be 2690; suppose their weight be 7 ounces; divide then according to the rule, the absolute weight 7 by the specifick gravity 2690, the quotient will give the total magnitude of all the fragments equal to $\frac{7}{2690}$ of a cubick foot; to reduce that to cubick inches, multiply

multiply these parts of a cubick foot by 1728, the number of cubick inches contained in a cubick foot, the product will be four inches and very nearly an half, which is the total magnitude of all the irregular fragments which were proposed to be measured: which is an easy and very exact way of obtaining the dimensions of several bodies which cannot be brought under the rules of geometry.

Another use which I mentioned of these hydrostatical tryals, was to discover in what proportion any two bodies are mixed together in a composition offered to be examined. The *data* requisite for this purpose, are the specifick gravities of the mixture and of the two ingredients; these are all of them to be obtained by the hydrostatical ballance, and are sufficient for the discovery, by the help of the following rules of proportion.

As the difference of the specifick gravities of the mixture and the lighter ingredient, is to the difference of the specifick gravities of the mixture and heavier ingredient, so is the magnitude of the heavier to the magnitude of the lighter ingredient; then as the magnitude of the heavier ingredient multiplied into its specifick gravity, is to the magnitude of the lighter ingredient multiplied into its specifick gravity, so is the weight of the heavier ingredient to the weight of the lighter.

The reason of this last rule is obvious enough from what has been said before, and the reason of the first may easily be found out by those who are qualified to understand it when demonstrated; I will therefore pass it over and propose an example.

Let it be the famous one of king *Hiero's* Crown. Suppose then the specifick gravity of the gold, which the king furnished his workman with for making the crown were as 19, and the specifick gravity of the crown as it was debased, were 16, and the specifick

gravity of the silver which the workman used for that purpose were 11, we are from these *data* thus to state our proportion according to the first Rule. As 5 the difference of 16 and 11, the specifick gravities of the mixture and lighter ingredient, is to 3 the difference of 16 and 19, the specifick gravities of the mixture and heavier ingredient, so is the magnitude of the heavier ingredient gold, to the magnitude of the lighter ingredient silver; by which it appears, that of the magnitude of the crown, 3 parts in 8 were silver. Then by the second rule we must say as 95, the product of 5 and 19, the magnitude and specifick gravity of the heavier ingredient gold, is to 33 the product of 3 and 11, the magnitude and specifick gravity of the lighter ingredient silver; so is the weight of the heavier ingredient gold, to the weight of the lighter ingredient silver. By which it appears that of the whole weight of the crown 33 parts in 128, or somewhat more than a fourth part were silver, if the circumstances were really such as we supposed them to be (*a*). Thus may the fineness of coins be examined and the proportion of alloy be determined without any detriment. And what has been said as to metals may be applied to other bodies and even fluids to very good purposes, if due caution be taken.

The third and last thing proposed was to give some instances of the usefulness of these enquiries to physicians, chymists, apothecaries, jewellers, goldsmiths, &c. Mr. Boyle has treated this subject very fully in his excellent *Medicina Hydrostatica*, I will therefore

(*a*) The rule abovementioned may be thus found out. Let the magnitudes of the gold and silver in the crown be *A* and *B*, and their specifick gravities as *a* and *b*; then, since the absolute gravity or weight of any body is compounded of its magnitude and specifick gravity, the weight of the gold is *aA*, of the silver *bB*, and of the crown $aA + bB = c \times A + B$ supposing *c* is the specifick gravity of the mixture. Hence $aA - cA = cB - bB$ and consequently $c - b : a - c :: A : B$, which is the rule abovementioned.

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transcribe some things from that book of his; a few will suffice to encourage the reading of it to those who have not yet done it, and many would be tedious to those who have. Having made it probable in his treatise of gems, that divers, if not most, of the real virtues of precious stones may in great part proceed from the quantities of metalline and mineral substances, that in the state of fluidity or softness were incorporated with the stony matter, which hardened afterwards into a gem, he was hence induced to suspect that divers boles, clays and other earths, that several minerals not looked upon as metalline, that several stones which are commonly neglected for want of beauty, may yet be endowed with considerable medicinal virtues, and perhaps with greater than the finer gems themselves; upon account of the great quantity of metalline and mineral substances, with which they might be impregnated whilst they were in *solutis principiis*. The method which he proposes for the exploration of fossils, is by finding their specifick gravities. For since the most pure and homogeneous kind of stones, are in gravity to water as about $2\frac{1}{2}$ to 1; and tin the lightest of metals is about 7 times heavier than water; if a stony substance be found to exceed in gravity that proportion of $2\frac{1}{2}$ to 1, it must be probable that it has in it some adventitious matter of a metalline nature, or is at least commixed with some mineral body more heavy than pure stone; and may therefore very probably be usefully applied to some medicinal purposes. He illustrates this matter by experiments made upon some substances which are found to be useful in physick; such are the *lapis hæmatites* or blood-stone, *lapis lazuli*, the load-stone, and *lapis calaminaris*, which he found to be in gravity to water respectively as 4.15, 3.00, 4.93 and 4.92 to 1.

A second use which he proposes of the hydrostatical way of enquiry, is to find out whether a body pro-

pounded as likely to be a stone of the mineral kind, be so indeed. Thus coral which, says he, some take to be a plant, others a lithodendron, but most reckon it among precious stones, is in gravity to water as 2.68 to 1, which favours the last opinion. Thus a pearl was found to be in gravity as 2.51; a *calculus humanus* was in gravity as 1.7; a bezoar as 1.5. These two last he thinks ought to be distinguished from common stones, being so much lighter, and he therefore chuses rather to call them animal stones than singly stones. A third use which he proposes, is to discover the resemblance or difference between bodies of the same denomination. A fourth use is to discern genuine stones from counterfeit ones, which may be of great help to jewellers. He instances in factitious coral and factitious gems, which he found out that way not to be genuine. A bezoar which in appearance seemed to be very fair and by no means a counterfeit, and had therefore a great price set on it, was the same way detected, being found to be as ponderous as a mineral stone of the same bigness, whereas it ought to have been nearly as light again. Thus mercury has been sometimes found not altogether 13 times and an half heavier than water; at other times it has been observed to be somewhat above 14 times heavier.

Now that we may observe this by the way, here may hence arise a notable difference in two weather-glasses at the same time and in the same place, if the mercury of the one be not of the same gravity with the mercury of the other; and the difference may amount even to a whole inch. Those therefore who publish registers of the weather, ought also to find out and declare to the world the specifick gravity of the quicksilver they make use of in their tubes.

After the same method may an estimate be made of the goodness of any of those substances that compose the *materia medica*, which is of great use to physicians,

ficians, chymists, apothecaries and druggists. Hence also may the goldsmith be assisted in judging of the fineness of his metals, and the merchant in his choice of sand gold and other precious commodities which are often counterfeited; and the miner may hence inform his judgment concerning the various substances he meets with under ground.

To conclude, this excellent philosopher, about the end of his book lets us know the high value he has for this method in the following words. “ As little skill
 “ as I have in hydrostaticks I would not be debarred
 “ from the use of them for a considerable sum of money; it having already done me acceptable service,
 “ and on far more occasions than I myself expected
 “ at first, especially in the examen of metals and mineral bodies, and of several chymical productions.
 “ And I have been able more than once or twice to
 “ undeceive artists and other experimenters, that *bona fide*, believed they had made, or were possessors
 “ of, *luna fixa*, as they call it, and other valuable
 “ things; and to make a judgment of the genuineness
 “ or falsity, and the degrees of worth and strength
 “ in their kind, of divers richer and poorer metal-
 “ line mixtures and other bodies, some solid and some
 “ liquid, whose fair appearances might otherwise
 “ have much puzzled, if not deceived me.”

L E C T U R E VII.

The several phænomena of the Torricellian experiment exhibited and explained.

HA V I N G exhibited the principal phænomena of the Torricellian experiment (*a*), I need not use many words to evince their dependence upon the gra-

(*a*) The principal phænomena of the Torricellian experiment, so called from the name of its inventor, are represented by FIG. 30,

vation of the air. Let us suppose that these things were altogether new to us, and laying aside all former prejudices in favour of any hypothesis, let us try whether a due consideration of the effects which we have seen, may not be sufficient to lead us into the knowledge of their causes. It appears at first sight to be altogether repugnant to the laws of hydrostaticks, that the quicksilver within the tube should be so much more elevated than that within the vessel into which

in the following manner. Having filled a glass tube with quicksilver, and covered its orifice with your finger, and inverted it, and immersed the finger in a vessel of quicksilver; upon withdrawing the finger from the orifice, the quicksilver will never wholly subside; if the tube be long enough, it will subside in part, till it rests at a certain altitude, generally between 31 and 28 inches; but if the tube be shorter than that altitude, called the standard, it will not subside at all.

It is further remarkable, if several tubes of various lengths, shapes and capacities be thus filled and inverted, that the surfaces of the contained quicksilver will rest exactly upon the same level in all, whether held upright or any way inclined. I say exactly, provided the bore of none of the tubes be too narrow, and due care be taken in filling them to expel all the little air-bubbles that adhere to their insides: which may be done by putting a slender wire into the tube and stirring it up and down; or, if the tube and quicksilver be very clean, as they ought, by leaving about an inch or less of the tube unfilled, by covering its orifice with your finger and by inverting it gently, that the air in the vacant part may ascend gradually along the tube, and sweep up the little air-bubbles along with it; and lastly by reverting it gradually to its former position and filling it up with quicksilver.

In this latter method take care that the large air-bubble may not ascend too quick, lest by its rushing against the crown of the tube with violence, it should break it. Also in emptying the tube, for fear of the like accident, take care first to incline in it, then to draw the orifice gently above the surface of the stagnant quicksilver, and immediately to immerse it again, so as to take in but little air at a time.

This is the way of making a weather-glass, which will be complete when placed in a common frame, having at the side a scale of three inches, divided into tenths as usual, and placed at the height of 28 inches above the surface of the quicksilver in the basin.

it was inverted. For imagining an horizontal plane to pass by the lower orifice of the tube through the the body of the quicksilver within the vessel, it will be evident that the part of the plane which is contiguous to and placed directly under the said orifice, has a greater weight of quicksilver incumbent upon it than any other equal part of the same plane. Now we have often seen in the former week, that fluids cannot possibly rest *in equilibrio*, whilst the equal parts of such an imaginary horizontal plane are unequally pressed upon, which nevertheless happens in our present experiments; therefore we must necessarily conclude, either that the general course of things is here interrupted and that these phænomena are a sort of miracle in nature, or that there is, contrary to what does at first sight appear, an equality of pressure upon every part of our imaginary plane. If there be that equality of pressure, it must proceed either from some increase of the lesser or some diminution of the greater, or perhaps from both; that is, there must either be some, as yet by us unheeded pressure, added to that of the quicksilver in the vessel, or some suspension of the quicksilver in the tube, whereby its excess of gravitation may be taken off.

Let it then be considered how either of these ways may be accounted for. If the equality proceeds from some pressure added to that of the quicksilver in the vessel, it must arise from something contiguous to the surface of the vesselled quicksilver. Since then the surface is contiguous to the air only, nothing but the pressure of the air can be that additional force I have hitherto been speaking of. The pressure of the air is therefore one of the causes we are to examine. We are also to consider of the other, and to enquire how the excess of pressure of the quicksilver in the tube may be suspended; and here I must needs confess myself to be utterly at a loss, not being able to imagine
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any cause sufficient for this effect, that shall at the same time agree with the rest of the appearances of nature. *Franciscus Linus* does indeed imagine he has found out what I despair of; let him then be answerable for his own conceit, which it may not be improper in this place to give you some account of; and I cannot do it better, since I have not *Linus's* book by me, than in the words of *Dr. Power*. His principles are these.

1. That there is an inseparability of bodies, so that there can be no vacuities in *rerum natura*.

2. That the deserted part of the tube is filled with a small film of quicksilver, which being taken off from the upper part of it, is both extenuated and extended through the seeming vacuity.

3. That by this extended film or rope, as he calls it, of dilated quicksilver, the rest of the quicksilver in the tube is suspended, and kept up from falling into the vessel.

4. That this funicle or rope is exceedingly rarefied and extended by the weight of the pendent quicksilver, and will, upon removal of that violent cause which so holds it, recontract itself into its former dimensions again, and so draw up what body soever it hath hold of along with it; as the effluvia of an electrick body, upon its retreat, plucks up straws or any other thing with it, that it is able to weild.

5. That this extension of the film of quicksilver is not indefinite, but hath a certain limit beyond which it will not be stretched; and therefore if the tube be of an exceeding great height, the quicksilver will rather part with another film and extend that, and so a third or fourth till it come to the standard of 29 inches where it rests, having not weight nor power enough to separate another film from it.

These are his principles, and that you may have a taste of the application he makes of them, I will add, that

that his reason why the quicksilver in a tube under 29 inches descends not at all is this, because it sticks with its uppermost surface so close to the top of the tube, that there is not weight enough to break that adhesion; the reason whereof is, because there is nothing to succeed in the room of the descending quicksilver, and therefore it firmly sticks there *ne daretur vacuum*.

In longer tubes it falls to that standard, because then the greater weight of the quicksilver is able to break that link of contiguity or adhesion, and therefore the uppermost surface of the quicksilver being sliced off, is dilated into a thin column or funicle, which supplies that seeming vacuity. Now, says Dr. *Power*, for the positive arguments to avouch his principles by, he has none at all, only what he fetches *a posteriore*, from his commodious solution of difficulties and salving the phænomena better than others have done. This is the hypothesis of *Linus*, and the only one I have met with that pretends to account for our phænomena by taking off the excess of gravitation of the quicksilver in the tube.

Thus much then we have hitherto established, that nature either suspends her settled laws for the production of these phænomena, or that there is some additional pressure communicated to the quicksilver in the vessel, which can be no other, as has been proved, than the pressure of the air which is contiguous to it; or lastly that the excess of pressure from the quicksilver in the tubes is by some way or other, which I confess I cannot discover, taken off or rendered ineffectual.

It is unreasonable to imagine that nature should forsake her wonted paths upon so trifling an occasion; it is certain we have no precedents to warrant such a suspicion; it has indeed been strenuously maintained by the schools, that nature does at all times suspend

pend any of her laws to prevent a *vacuum*, to which they confidently tell us she has a most dreadful aversion. Now by nature they must mean, if they mean any thing, either the author of all created beings or the creatures themselves; if they would be understood in the first sense, they do unavoidably charge omniscience its self with incogitancy, supposing him so to have created the world as continually to stand in need of miracles for its preservation; it being in their own power as often as they please, to make a trifling experiment to put him to the necessity of interposing to hinder a *vacuum*. If they mean by nature the creatures themselves, then they must of necessity fall into another absurdity, whilst they suppose brute matter to be intelligent, and to put its self into action in pursuit of some determinate end.

This I presume may be sufficient to expose that gross opinion concerning a *fuga vacui*, supposing it could account for our experiments, which it cannot do by any means. When the length of the tube was less than 30 or 29 inches or thereabouts, the quicksilver as we saw did not ascend, and thereby desert the top of the tube, so as to leave a vacuity behind it; here indeed it possibly might be pretended that nature went out of her way to prevent a *vacuum*; why then was she not equally concerned to do it when the tube was longer? It must at last be said that her power is limited and kept in by certain determinate laws.

There remains then but two ways of explaining the phænomena we are concerned with. The pressure of the air upon the quicksilver in the vessel, or some unknown cause which takes off the excess of pressure from the quicksilver in the tube. Let us now try whether either of them may possibly be excluded, so that at length we may be certain what to fix upon.

We may make use of that ingenious device of Mr.

Azouk

Azout as an *experimentum crucis* in our present enquiry (*a*). We are chiefly to attend to what happened in the upper tube and cistern. It was evident that the quicksilver which remained in the upper cistern, after opening the orifice of the lower tube, was free from the contact of the air; if therefore the elevation of the quicksilver in the tubes in the former experiments, did proceed from the pressure of the air

(*a*) The instrument for trying *M. Azout's* experiment consists of several parts. In FIG. 31, *ab* is the lower bason, *bc* the lower tube, *cdef* the upper bason, whose bottom part is skrewed at *c* into a hollow brass collar cemented round the top of the lower tube, and *efg* is the upper tube, put through a brass collar cemented to it at *f* and skrewed into the upper part of this bason. This tube reaches almost to the bottom of the bason, covered over with hard cement; through which, towards one side, there passes a small pipe *bi*, so bent, underneath the cement, towards the middle of the bason, as to reach down towards the orifice of the under tube *cb*.

The manner of making the experiment is this. The bottom of the lower tube is surrounded with two parallel collars of hard cement, having a neck between them, for the convenience of tying a piece of wet bladder very tight over this orifice of the tube. Having taken off the upper cistern and tube, first fill the lower tube with quicksilver, well purged of air-bubbles; then skrew on the upper bason, and fill it with quicksilver, which will also fill the included pipe *bi*; then skrew in the upper tube and fill it also with quicksilver, well purged of air-bubbles, up to the very top, and tie a wet bladder over it in the same manner as below.

The instrument being thus filled, with a small penknife immersed in the quicksilver in the lower bason, cut flits in the lower bladder, for the quicksilver in the tubes to descend through; by which means the upper tube will first be evacuated and then the upper part of the upper bason, as low as the upper orifice of the bended pipe; then will this pipe be next evacuated and the upper part of the lower tube, so far as to leave no more in it than a column equal to the standard altitude, as in the Torricellian experiment made with a single tube.

But upon unskrewing the upper tube, so as to admit some air gradually into the upper bason, the quicksilver will ascend to the standard altitude in the upper tube, and descend quite to the bottom of the lower tube. Lastly upon pricking the upper bladder with a pin, the admitted air will depress the quicksilver, in this tube also, to the level of that in this bason,

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upon the quicksilver in the cistern, because in this upper cistern there was no such pressure, there ought to be no such elevation; but if the elevation of the quicksilver in the former experiments did depend upon some other unknown cause, by which the excess of pressure from the quicksilver in the tube was taken off, then ought the quicksilver in the tube to remain also elevated in this experiment, all circumstances being the same here as in the former, excepting that the air is here excluded from the surface of the quicksilver in the upper cistern. Since then in this decisive experiment we saw that the quicksilver in the upper tube was not elevated as in the former experiments, but fell down to the level of that which was contained in the cistern, we may very securely from hence conclude, that its elevation in the former experiments was not owing to any other cause, but to the pressure of the air only.

For a further confirmation of this conclusion, which yet needs not any, we may observe, that as the air was permitted gradually to enter into the upper cistern, so was the quicksilver gradually elevated in the upper tube, till it had attained its standard altitude of about 29 or 30 inches; and at the same time the quicksilver in the lower tube was depressed gradually, till it was reduced to the level of that in the lower cistern. For the air being suffered to press equally upon the quicksilver in the lower tube and upon that in the lower cistern, there is now no reason why it should be any longer elevated, as it was when the air pressed only upon that within the cistern. You will hereafter meet with still further proofs and confirmations of the air's pressure.

It is now our business to account for that observation, which was made upon these experiments, that the perpendicular altitude of the quicksilver in the tube above that in the cistern is constantly the same, whatever be the length, wideness, figure or situation
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of it. We have seen already that the pressure of the air is that additional force which counterballances the excess of gravity from the quicksilver in the tube; it will follow from hence that, that part of the upper surface of the quicksilver in the cistern which is contained within the orifice of the tube, is just so much, neither more nor less, pressed upon by the quicksilver in the tube, as any other equal part of the same surface is pressed upon by the weight of the column of air incumbent on it.

We saw in the last week, that the pressure of all fluids upon any proposed plane was according to their altitudes; that as long as the altitude was the same, the pressure was also the same, though the quantity of the pressing fluid was never so much altered, either by being contained in a vessel differently figured or differently inclined. Therefore in all the cases of the experiments which have been made this day, the pressure of the quicksilver contained in the tubes, however figured or situated, is every where the same, and every where equal to the pressure of the air upon the other equal parts of the surface of the quicksilver in the cistern, because, as we saw, the altitude is always the same. The elevation of the quicksilver always to the same height, ought not then to be urged as an argument against what we have proved, that the gravitation of the air is the cause of the phænomena in the Torricellian experiment, since we have shewn it to be a necessary effect of that cause.

Let us now proceed to those experiments which were made to determine the force requisite to bear up the inverted tube (*a*). We observed that the force

(*a*) In FIG. 32, *ab* represents the Torricellian tube immersed in a basin of quicksilver, and suspended at the beam of a ballance by the string *a*, lapped about the end of the tube and fastened to it with hard cement. The tube being counterpoised with weights in the opposite scale, its orifice plays freely within the quicksilver in

was

was, as nearly as we could estimate it, equal to the weight of the tube and the quicksilver contained in it; abating the weight of so much quicksilver as was equal in magnitude to the part immersed. This phænomenon does at first view seem to disagree with what we have hitherto advanced, and to favour the funicular hypothesis; accordingly the patrons of that hypothesis have not been wanting to make use of it for their purpose. For if the pressure of the air upon the surface of the quicksilver in the vessel, be the true cause of the elevation of the quicksilver in the tube, it should seem that the weight of the quicksilver in the tube, being sustained by that pressure of the air, ought not in the least to be perceived by the hand of him who holds up the tube; and that he ought to be sensible only of the bare weight of the tube. Since then the weight of the quicksilver, as appears by our experiments, does also seem to load the hand, it may perhaps be suspected that the quicksilver is connected to the top of the tube by *Linus's* rope.

But we ought to observe that this weight, which at first sight one would be apt to ascribe to the quicksilver contained in the tube, is not in reality the weight of the quicksilver, that being unquestionably supported by the pressure of the air upon the stagnant quicksilver of the cistern below, but is the weight of the column of air incumbent upon the crown of the

the basin, and is hindered from ascending accidentally above it, by a table placed under the opposite scale. With your finger immersed in the quicksilver, having covered the orifice of the tube and shut up the column within it, take it up and incline it gradually till it be quite inverted, then putting the closed end into the quicksilver in the basin, hang the open end upon the balance by the string *b*, and the beam will again be in equilibrio as before.

$\alpha \beta$ represents a like tube, shorter than the standard altitude, which being hung at the balance by the string α or β at either end, will also counterpoise the same weight in both positions. I believe Dr. Wallis was the inventor of this experiment.

tube,

tube, which is equivalent to the weight of the quicksilver contained in the tube, and which we therefore unwarily are apt to look upon as the very weight of that quicksilver it self. For the weight of that air ought in this experiment to be perceived by the balance, or by the hand of him who held up the tube, since it is not counterballanced and so taken off by an equal pressure from below, as it is when the tube is empty; that counterballance being now otherways employed in bearing up the quicksilver within the tube.

There have been several warm disputes, but to little purpose, about the space in the top of the Torricellian tube, which is deserted by the descending quicksilver, some holding it to be an absolute vacuity, others denying that there is or can be any such thing in nature. Their *materia subtilis* is always ready upon any difficulty, and they employ it a thousand ways as occasion requires. This we may securely affirm, and it is sufficient for our purpose, that the deserted space, if it be not altogether free from the common air we breathe, yet is so as to all sense; which we may be certain of by inclining the tube; for the quicksilver will again repossess the space it had formerly quitted, which it could not do, were that space taken up by the air. We may indeed sometimes, upon inclination, perceive some air still lurking behind, but the quantity of it is generally so small, if care be taken in making the experiment, that it deserves not to be regarded.

LECTURE VIII.

Mr. Pascal's imitation of the Torricellian experiment by water; other experiments of the like nature with fluids variously combined; the pressure of the air shewn by experiment to be different at different altitudes from the surface of the earth.

I PROCURED the apparatus for that chargeable and troublesome experiment of Mr. *Paschal* (a) rather as a curiosity than as absolutely necessary to our purpose; for since the pressure of the air was able to keep the quicksilver in the Torricellian tube, elevated to the altitude of about 29 or 30 inches, as we yesterday saw, we might without making the experiment safely conclude, that the same pressure of the air would be sufficient to keep water elevated in the Pascalian tube to an altitude about 14 times greater; that is, to about 34 feet. For since quicksilver, as we found by the hydrostatical ballance, is about 14 times specifically heavier than common water, so the altitude of water requisite to counterballance the pressure of the air, ought to be about 14 times as great as the altitude of quicksilver requisite for the same purpose. But matter of fact is always more convincing than any reason how well soever it be grounded, and what we see with our eyes is always more satisfactory than any relation of what has been done by others. We have therefore ventured to repeat that noble experiment, which had also been tried by others with success, though not very often by reason of the great difficulty in managing it.

Mr. *Pascal* is said to be the first that attempted it. The learned and industrious Jesuit *Gaspar Schottus* has,

(a) The best way of trying this experiment is sufficiently described in the sequel of the lecture.

in his *Technica curiosa*, given us an account, from a letter of Mr. *Roberval*, of the motives which chiefly induced Mr. *Pascal* to make this tryal. He tells us from that letter, that Mr. *Pascal* did at Roan in Normandy exhibit this experiment with water and wine, in tubes of crystal glass 40 feet long, which were fixed to the mast of a ship, that was contrived to be raised and depressed as need required. He also informs us that the occasion of having recourse to tubes of that length, was, that when several learned men saw the quicksilver in the Torricellian experiment, when it deserted the upper part of the tube, so to descend as always to possess in the lower part of the tube, the altitude of 2 Paris feet and $3\frac{1}{2}$ inches nearly, measured perpendicularly from the surface of the quicksilver in the vessel underneath, they divided into different opinions; and some of them who were peripateticks asserted, that in the upper part of the tube, deserted by the quicksilver, there was contained some spirits evaporated from the quicksilver, which being rarefied did fill that part, thereby helping nature now put to her shifts, against her mortal enemy a *vacuum*. Now Mr. *Pascal*, that he might plainly convince these men of their error, procured (says *Roberval*) crystal tubes of 40 feet in length to be tied to a mast, and engines to be applied as was said before; and having fixed upon a day and a spacious place near the glass-house, he invited all to be present to see wonders.

Now Mr. *Pascal* had privately made a calculation of water and wine, compared with quicksilver as to their gravities, that thence he might find out the altitude due to each of them, so as they might equi-ponderate; and he found, that taking 2 Paris feet and $3\frac{1}{2}$ inches for the altitude of quicksilver, the altitude due to the water must be 31 feet and about $\frac{1}{2}$; and in like manner the altitude due to the wine must be 31 feet and $\frac{2}{3}$ nearly. Then before he opened any

thing of his design, by questioning them he easily made them confess that there was certainly a greater quantity of spirits in wine than in water; so that if the experiment could be made with those liquors, the wine would leave a greater space in the top of the tube than the water, provided the tubes were of the equal lengths. This being allowed him, the mast was shewn with the tubes tied to it, which being filled, the one with water, the other with wine, and their orifices being closed, the mast was erected, and vessels were applied to the orifices, the one filled with wine, the other with water, into which the orifices were immersed, the tubes still remaining full till their orifices were opened; which when done, the liquors contained in the tubes did so descend, as that after they came to rest, the altitude of the water in its tube, above the surface of the water in the vessel below, was 31 feet and about $\frac{1}{5}$, but the altitude of the wine was somewhat greater, being 31 feet and about $\frac{2}{3}$; the upper parts of both tubes remaining to appearance void of any thing, as is usual in the Torricellian experiment.

The liquors in the tubes were afterwards changed, that being filled with water which was before filled with wine, and that with wine which was before filled with water; notwithstanding this, no alteration was observed as to the altitudes. Thus did he confute his adversaries from their own concessions, shewing them, if their hypothesis were true, that a greater space was necessarily occupied by the spirits of water than by those of wine, and consequently that water was more spirituous than wine; contrary to all reason and experience.

Let us now come to those experiments which were made by combining two different fluids in the same tube. The trials we made were with quicksilver and water, quicksilver and air, water and air, and these
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are sufficient to let us understand what the event would be if any other fluids be made use of. We have seen that quicksilver is elevated by the pressure of the air to about 29 inches and $\frac{1}{2}$, and water to about 34 feet. It is easy then to understand that a mixture of quicksilver and water, must needs be raised by the same pressure to some intermediate altitude; what that altitude will precisely be, must be determined by the particular quantity of water we employ in the experiment.

Let us suppose that the quicksilver we make use of is 14 times specifically heavier than the water, and that the state of the weather is such, that the weight of the air is equipollent to 30 inches of that quicksilver, as it often happens to be; putting then 28 inches of water into the tube, we may thus compute beforehand what the event will be: 28 inches of water are equal in weight to 2 inches of quicksilver, 28 inches being equal to 14 times 2 inches of quicksilver, as the specific gravity of quicksilver is 14 times greater than the specific gravity of water; but the air was able to sustain 30 inches of quicksilver by supposition; since then these 28 inches of water are equal but to 2 of the 30, there remains 28 inches more of quicksilver to equal the pressure of the air. The whole compound then of quicksilver and water will be sustained at the altitude of twice 28 or 56 inches.

Suppose again that 14 inches of water were placed in the tube; then because 14 inches of water are equivalent to one of quicksilver; taking that one inch from 30 as before, there will remain 29 due to the quicksilver, so that the whole compound of quicksilver and water together will stand at 43 inches.

If quicksilver and air be to be combined together in the same tube, it will be much more difficult to make an estimate of the altitude at which the quicksilver will stand, than in the former case; the problem

is properly algebraical, and he who will solve it must do it analytically, for which reason I shall omit it in this place and invert the problem. Instead of computing at what height the quicksilver will stand for any proposed quantity of air lodged in the top of the tube, I will here shew how to determine the quantity of air which being placed in the upper part of the tube, will cause the quicksilver to rest at any given altitude less than the Torricellian standard.

The rule may be thus stated: as the standard altitude of the quicksilver in the Torricellian tube at the time of making the experiment, is to the defect of the proposed altitude from that standard, so is the length of that upper part of the tube which is to be left free from quicksilver after inversion, to the length of that part of the tube which is to be left free from quicksilver before the inversion.

I will first endeavour to make this rule clearly understood by an example or two, and then give the reason of it. Suppose at a time when the altitude of the quicksilver in the Torricellian tube or barometer is 30 inches, it were required to fill a tube of 36 inches partly with quicksilver and partly with air, so that after inversion the altitude of the quicksilver may be 20 inches. Let us suppose that when the tube is inverted, one inch of it is immersed in the stagnant quicksilver of the vessel below, so that there remains but 35 inches above the surface of the stagnant quicksilver; the defect of the proposed altitude 20 inches from the standard altitude 30 inches, is equal to 10 inches; the length of the upper part of the tube which is to be left free from quicksilver after inversion, is 15 inches; for there being but 35 inches of the tube extant above the surface of the quicksilver in the vessel, and 20 of these 35 being possessed by the quicksilver, there will remain 15 free from the quicksilver; we must say then, by the rule, as 30
inches;

inches, the standard altitude, are to 10 inches the defect of the proposed altitude from the standard, so are 15 inches, the length of the upper part of the tube to be left free from quicksilver after inversion, to 5 inches, the length of that part of the tube which is to be left free from quicksilver before the inversion. If then we fill the tube with quicksilver excepting 5 inches which we leave to be possessed by the air, then closing the orifice we invert it, these 5 inches of air will readily ascend to the top of the tube; afterwards immersing the lower end an inch deep under the quicksilver in the vessel, when we uncover the orifice, the quicksilver will immediately fall down to the altitude of 20 inches, which was the thing proposed to be effected; and the air which did just before possess only 5 inches, will now upon the retreat of the quicksilver dilate itself to 15.

In this example the tube was 36 inches long, which is more than the standard; suppose now it were but 24 which is less than the standard; let an inch as before be immersed in the vessel, there will then be but 23 extant, and let it be proposed to find out how much air must be left in the tube before inversion, to make the quicksilver rest at the altitude of 18 inches after inversion. The defect of 18 inches from the standard is 12 inches; the upper part of the tube which is to be free from the quicksilver, is 5 inches, 18 of the 23 inches extant being to be possessed by it. Say then as 30, the standard, is to 12, the defect of the altitude proposed from the standard, so is 5, the part free from quicksilver after inversion, to 2, the part to be left free from quicksilver before inversion. If then the tube be filled with quicksilver excepting 2 inches, after inversion the quicksilver will rest at the altitude of 18 inches.

In order to make out this rule we must observe, in the first place, that there is a spring or elastical power

in the air we live in; by which I mean that our air either consists of, or at least abounds with parts of such a nature, that in case they be compressed and thereby reduced into lesser dimensions, either by the weight of the incumbent part of the atmosphere, or by any other force, they endeavour as much as in them lies, to free themselves from that pressure, and to regain their former dimensions, by bearing against the contiguous bodies that keep them in. This is what any one may observe in a blown bladder; the air contained in it may by the force of his hands be reduced into a lesser space, but then as soon as the force is removed, it will immediately expand itself as before, and you may perceive even whilst you compress it, a very great endeavour to free itself from the violence you offer to it.

In the second place we may observe, that this elastical or expansive power of the air is equivalent to the force which compresses it; for were it less, it would still yield to a further degree of compression, and were it greater, it would not suffer itself to be so much reduced, action and reaction being always equal. It follows from hence, that the elastick power of any small parcel of the air we breathe, is equivalent to the weight of the incumbent part of the atmosphere, that weight being the force which confines it to the dimensions it possesses. Though this assertion may at first view seem a paradox, yet, as was said before, if the elasticity or spring of this small portion of air, were not so great as the weight of the incumbent part of the atmosphere, it would yield to that weight and permit itself to be confined in narrower bounds.

In the third place we may take notice, that by a greater compression, the air is still reduced into lesser dimensions, and on the contrary, its dimensions are enlarged as its compression is diminished. We cannot
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by any reasoning alone find out what sort of proportion the dimensions bear to the compressions. The Author of nature might have ordered these things otherwise than he has done by infinite variations; we must therefore by experiments try what is the constitution of nature. I design therefore at our next meeting to attempt this business, and I hope then to demonstrate, that the space which any proposed quantity of air at any time possesses, is reciprocally as the force which compresses it.

When I say the space is reciprocally as the force, I mean that the space is diminished in the same proportion in which the force is increased, and increased in the same proportion in which the force is diminished: thus a double force reduces the air into half the space, a triple force reduces it into a third part of the space it possessed before; so half the force permits the air to expand itself into double the space, and a third part of the force permits to expand itself into a space triple of that which it possessed before.

Now the force of elasticity, being as I have already proved, equal to the force of compression, it will follow that the force of elasticity is reciprocally as the space which the air takes up: which property I will at this time take for granted, and proceed to make out the truth of our rule.

In FIG. 33 and 34, let ae be the tube proposed, bc the altitude at which the quicksilver is to stand, bd the standard altitude in the Torricellian experiment. The pressure of the atmosphere upon the surface of the vessel below is, in the Torricellian experiment, ballanced by the column of quicksilver bd , or bc and cd together; in our present experiment it is ballanced by the weight of the column bc and the pressure downwards of the air ec , contained in the top of the tube, upon the upper surface c of the elevated quicksilver; which pressure arises not from
the

the weight of that included air, for that is altogether inconsiderable, but from its elasticity or endeavour to enlarge its dimensions, which it can no otherways do but by depressing the quicksilver cb ; the weight of the columns bc and cd is therefore equivalent to the weight of bc and the elastick force of the air ce , both being equivalent to the pressure of the atmosphere upon the surface of the vessel. The weight of the column of quicksilver cd is therefore equivalent to the elastick force of the included air ce .

Let ef be the space which that air in its natural state did possess, when it was first lodged in the top of the tube, before it had depressed the quicksilver to c , and by its spring expanded itself to its new dimension ec . Its elastick force whilst it was in its natural extent ef , was equivalent to the pressure of the atmosphere, as has been already proved; and therefore equivalent to the weight of the column of quicksilver bd .

Thus far then have we hitherto proceeded; we have found that the elasticity of air in the space of ec , is equivalent to the weight in cd ; that the elasticity of the same air in the space ef is equivalent to the weight of bd ; the elasticity in ef therefore is to the elasticity in ec as bd to dc .

Now by the property of the air which I told you was to be proved to morrow, that the elasticity is reciprocally as the space, it follows that the elasticity of the air in ef , is to its elasticity in ec , as the space ec to the space ef . Therefore as bd is to dc , so is ec to ef ; that is, as the standard altitude in the Torricellian tube at the time of making the experiment, is to the defect of the proposed altitude from that standard, so is the length of the upper part of of the tube which is to be left free from quicksilver after the inversion, to the length of that part of the tube which is to be left free from quicksilver before the

the inversion; and this is what I undertook to make out (b). The same rule will hold good with a small alteration, if instead of quicksilver we would combine water and air together in a tube of any proposed

(b.) FIG. 33, 34. Having ef to find ec or cb , is the converse of the foregoing proposition, and as the author observed above, is to be solved analytically.

By the allowed property of air, that its elasticity is reciprocally as the space it possesses, we had $ec : ef :: bd : dc$, or $ec \times dc = bd \times ef$; which shews that the question abstractly considered, is only to find a point c , in a given line ed produced, at which the rectangle under ce and cd , shall be equal to the given rectangle under bd and ef . Now since the sides ce , cd are related both alike to their difference de , and consequently both determinable by a like analysis, it would be arbitrary to seek either of them rather than the other; which intimates, that a simpler and better way will be, to bisect their given difference de in g , FIG. 35, and to seek their half sum gc .

Hence we have $ce = cg + gd$ and $cd = cg - gd$, and $ce \times cd = cg + gd \times cg - gd = cg^2 - gd^2 = bd \times ef$ by the condition of the problem: which gives this Theorem, $cg^2 - gd^2 = bd \times ef$, or, $cg = \sqrt{gd^2 + bd \times ef}$.

We had an example in pag. 86, where $bd = 30$ inches, $de = 5$, and $ef = 5$. Here $\frac{1}{2} de$ or $dg = 2.5$. and, by the Theorem, $cg = \sqrt{6.25 + 150} = 12.5$; whose difference $cd = 10$ and sum $ce = 15$, and the column $cb = 20$ inches.

But instead of the Theorem for arithmetical computation, if a geometrical construction be desired; in FIG. 35, 36, to the standard altitude bd add $db = ef$, and upon the diameter bb describe a semicircle bib , cutting in i a line di drawn perpendicular to the tube; then bisecting de in g , and joining gi , a circle described with the center g and semidiameter gi , will cut the tube in the point c where the surface of the column of quicksilver will rest.

This will appear from the Theorem $cg^2 = gd^2 + bd \times ef = gd^2 + bd \times db$ by construction, $= gd^2 + di^2$ by the known property of the semicircle bib , $= gi^2$ by the property of the right angled triangle gdi .

But if a synthetical demonstration be desired, let the circle ci cut the produced tube in k , then $bd \times ef = bd \times db$ by construction $= di^2$ by the known property of the circle bib , $= dc \times dk$ by the like property of the circle ci , $= dc \times ec$; and by resolving

length.

length. We must then say as the standard altitude of the water in the Pascalian experiment, which is commonly about 34 feet, is to the defect of that proposed altitude of the water from the standard, so is the length of the upper part of the tube which is to be left free from water after inversion, to the length of that part of the tube which is to be left free from water before inversion.

We may now go on to the experiment that was made at the top and bottom of the Observatory, which affords us as sensible an argument for the air's pressure as can well be desired, and of the difference of that pressure at different altitudes (*c*). I must confess I cannot see any objection that may with the least

the first and last rectangles into a proportion of their sides, we have $ec : ef :: bd : dc$; therefore, by the property of the air's spring, the surface of the quicksilver will rest at *c*.

The problem abstractly considered, has two answers, because the circle *c i k* cuts the produced line *de* in two points; whereof *k* has this property in our particular problem, that if the space *e k* be filled with a column of quicksilver, having a vacuum above it, its upper surface will rest at *k*, as that of the lower column does at *c*; because the spring of the air included in the space *ce*, is equipollent to the weight of a column whose length is *ek* or *cd*.

(*c*) FIG. 37. Having with a red hot iron burned a moderate hole length-ways through a cork, and squeezed it hard into a glass bottle, whose bottom is covered with water about an inch deep; and having run a glass tube, open at both ends, through the hole in the cork, quite down into the water, and covered the top of the cork with cement, to hinder the air from passing by the sides of it; with your mouth applied to the top of the tube, force some air downwards into the water, till it rises in bubbles above its surface, and increases the quantity of air within; whose spring, by pressing stronger now than before on the surface of the included water, will raise it in the tube above the mouth of the bottle. Then having placed the bottle in any convenient vessel, and covered it all over with salt or sand, to keep the included air of the same temperature; after the surface of the elevated water is quite settled at any place of the tube, mark it by tying a thread about the tube or otherwise. Then having carried the vessel up to the highest place at hand, you will find the water risen higher in the tube, and resting above the mark: which plainly shews that the pressure of the air
degree

degree of probability be urged against it. I shall not therefore go about to obviate any. What I think most pertinent to observe concerning it, is this, that we are not only from hence convinced of the weight of the air, but may hereby also determine the proportion of its specifick gravity to that of water. The difference in height of the two places in which we made the experiment is 54 feet, and that difference in height caused the difference of $\frac{3}{4}$ of an inch in the height of the water. By which it appears that a column of water of $\frac{3}{4}$ of an inch or $\frac{1}{12}$ of a foot, is equi-ponderant to a column of air of 54 feet having the same basis. Therefore the gravity of water is to that of air, as 54 to $\frac{1}{12}$ or as 864 to 1.

We might have made with the Torricellian tube an experiment like this, to shew the different pressure of the air at different distances from the surface of the earth, had the Observatory been much higher than it is. At the altitude of 54 feet the ascent of the quicksilver would be too small to ground any thing upon, being but about $\frac{1}{12}$ of an inch. It was therefore necessary to make use of the contrivance you have seen, to supply the defect of some very high mountain, upon which had any such been near us, we might have observed a sensible alteration even with the Barometer. Such an experiment was formerly made at the desire and by the direction of Mr. *Pascal*, in the year 1648, upon the Puy de Domme, a very high mountain in France. It was then observed that in ascending 3000 Paris feet, the quicksilver in the tube fell

upon the surface of the water, is now less than it was below-stairs. For the air within the bottle being of the same temperature as before, presses by its spring upon the included water within the same force; which being ballanced in part by a column of water in the tube, now longer and heavier than the former, must have its total ballance made up by the weight of a lighter column of air incumbent upon the heavier column of water.

down

down 3 inches and $\frac{1}{8}$ of an inch. To reduce this to English measure, we may say that ascending 3204 English feet, the height of the quicksilver was abated 3 inches and $\frac{1}{8}$ of an inch. Another experiment like this was made by Mr. *Caswell* upon Snowdon Hill in Wales; he found that the height of 3720 feet abated the quicksilver 3 inches and $\frac{8}{10}$.

It may not be amiss here to add the result of a computation which I made of the weight of all the air which presses upon the whole surface of the earth. If this weight were to be expressed by the number of pounds it contains, that number would be so large as to be in a manner incomprehensible. I will therefore make use of another way of expressing it, by determining the diameter of a sphere of lead, of the same weight with all the air which presses upon the whole surface of the earth. Now that diameter was found to be very nearly 60 miles long. If any one has a desire to make this calculation after me, he may proceed upon these grounds. That the weight of a column of air reaching to the top of the atmosphere, is most commonly equal to a column of water having the same basis, and the altitude of 34 feet; that the semidiameter of the earth is equal to 20949655 feet, and that the specifick gravity of water is to that of lead as 1000 to 11325.

LECTURE IX.

The density and spring of the air proved to be as the force which compresses it, and from hence an enquiry is made into the limits and state of the atmosphere.

IT was proved yesterday that the air has a spring or elastical power, by which it constantly endeavours to expand itself; and that the force of that spring is always equivalent to the force by which the
air

air is compressed; but we did at the same time take for granted that the space which the air possesses is reciprocally as that force, and consequently its density directly as the same. Let us now try by making experiment, whether the assertion will hold true. The air may be either more rare or more dense, than it is in that constitution of it, which we commonly, but perhaps somewhat improperly, call its natural state. We will therefore by two different sets of experiments make our trials upon it, first when it is more and afterwards when it is less expanded than as we usually breathe it.

Let ae in FIG. 33, be a tube hermetically sealed at the end of e and open at the end a ; placing the end e downwards, if we fill the whole tube with quicksilver excepting a certain space, which we leave to be possessed by the air, then stopping the orifice a and inverting the tube, we permit the included air to ascend into the space ef , afterwards immersing the end a into a vessel of quicksilver, we open the orifice, which was before closed, the quicksilver will descend to c , and the air ef will thereby expand itself into the space ec . Now if bd be the standard altitude in the Torricellian experiment, you may remember it was yesterday proved that the force by which the air was compressed, whilst it was contained in the space ef , was equivalent to the weight of the column of quicksilver bd ; and the force with which it was compressed whilst in the space ec , was equivalent to the weight of the column of quicksilver dc . If then we find by making experiment that the space ec is constantly to the space ef , as the force which compresses the air in the space ef , is to the force which compresses the same air in the space ec , that is, as the weight of the column bd to the weight of the column dc , or as the length bd is to the length dc , we may safely conclude that the space which the air when rarefied possesses, is reciprocally as the force which compresses it.

To examine also whether the same proportion holds

true for condensed air, we may make use of a tube bent up like that in FIG. 38, whose extremity n is supposed to be open, but g to be hermetically sealed up. Pouring in then just so much quicksilver as will fill the bottom ik , so as to shut up the air ig from making its escape, if the surfaces of the quicksilver at i and k be both upon the level, we may conclude that the pressure of the air ig upon the surface i , is equivalent to the weight of that part of the atmosphere which presses upon k ; and therefore that the weight of a column of quicksilver of the standard altitude, is equivalent to the force which compresses the air ig . After this if we pour in more quicksilver at n till it ascends in the longer leg to m , we shall at the same time perceive it to rise in the shorter leg to b , and therefore the air which before did possess the space ig , will be condensed and reduced to the space bg . Now in this case it is evident that the force which compresses the air into the space bg , is equivalent to the weight of the column of quicksilver lm , besides the weight of a column of the standard altitude, if l be upon the level with b . If then we find upon trial, that the space bg is constantly to the space ig , as the force which compresses the air whilst it is contained in the space ig , is to the force which compresses the same air whilst it is contained in the space bg , that is, as the standard altitude is to the standard altitude and the altitude lm together, we may also conclude, that the air when condensed does always possess a space which is reciprocally as the force which compresses it.

Let us try then whether the event will nearly answer what we expect. We may be certain there will be some small difference after all the cautions we can possibly take, unless the bores of the tubes ec and gi , FIG. 33, 38, be truly cylindrical, which seldom or never happens; and the cause of that difference which may arise is this, that we suppose the spaces ef and ec , gi and gb to be to each other as their lengths,

which

which supposition is erroneous unless the tubes be perfect cylinders.

Having yesterday made it appear from reason, that the spring or elastick power of the air is as the force which compresses it, and having this day, as far as the unavoidable irregularity of tubes would permit us, shewn by several experiments that the density is also as the said force, the space it possesses being always reciprocally as that force; we are now furnished with sufficient *data* to make our enquiries concerning the limits of the atmosphere, and to determine its state, as to rarity, at different elevations from the earth's surface.

If the air were of the same consistence as to its rarity or density at all altitudes, it would be no difficult thing to set bounds to it. We collected from the experiment which was yesterday made at the top and bottom of the Observatory, that the specifick gravity of water is about 850 times greater than the specifick gravity of air (which thing will hereafter be further examined by an experiment particularly fitted for that purpose) and in the foregoing week we found by the hydrostatical ballance, that quicksilver is about 14 times heavier than water; it follows then of consequence that quicksilver is 14 times 850 degrees heavier than air, that is, 11900 times heavier. We have seen by the Torricellian experiment, that a column of quicksilver of $29\frac{1}{2}$ inches is usually a counterpoise to a column of air, having the same base and reaching to the top of the atmosphere; if therefore the air be every where of the same density as it is here below, its altitude ought as many times to exceed the height of $29\frac{1}{2}$ inches (which is the height of an equi-ponderant column of quicksilver) as its specifick gravity falls short of the specifick gravity of quicksilver; that is, the height of the atmosphere ought, upon the supposition of an every where uniform density,

to be 11900 times $29\frac{1}{2}$ inches, or somewhat above $5\frac{1}{2}$ miles.

But it may be easily proved that this supposition does in no wise take place. For since every region of the air is compressed by that part of the atmosphere which is superior to it, and since the higher parts have a lesser weight incumbent upon them than the lower, and since the density of the air is every where as the force which compresses it; it will follow of necessity that there is still a greater rarity of the air as it is further distant from the surface of the earth. How far the air may possibly admit of rarefaction and condensation, has not yet, that I know of, been determined by any one. Mr. *Boyle* has observed that it may be so dilated as to become 10000 times rarer than it is in its natural state. Dr. *Halley* says that he himself has seen air compressed so as to be 60 times denser than it is as we commonly breath it; and Monsieur *Papin* relates that he was a witness that Monsieur *Huygens* did once in a glass vessel compress air to the same degree before the glass was broken; yet never could any experimenter determine how much farther air might possibly be rarefied or condensed. However it is certain there are in nature some limits which cannot be exceeded. No condensation can reach so far as to cause a penetration of parts; and if the rarefaction of the air be still greater, as its distance from the surface of the earth increaseth, its spring will at length be so weakened, that the force with which every particle of it endeavours to tend upwards, from the particles which are next below it, will be weaker than the force of its own gravity which endeavours constantly to detain it. The rarefaction of the air must therefore be bounded where these two opposite forces come to ballance each other.

Though this be certainly true that the air cannot possibly expand itself beyond a certain measure upon account

count of its gravity, yet since men have not hitherto been able to set any bounds to its utmost expansion, it is equally certain, that we cannot possibly define the limits of the atmosphere. For as the air may be more and more rarefied, so will the same quantity of it, which equals the weight of about 30 inches of quicksilver, be contained in a greater space, and thereby those limits be so much the wider.

Notwithstanding this seeming difficulty, we may still collect how much the air is rarefied at any proposed altitude from the surface of the earth, after the following manner.

In FIG. 39, let $xa\alpha px$ represent a vessel reaching from the surface of the earth $a\alpha$ to the top of the atmosphere x ; and let us imagine the side $a\alpha$ divided into inches ab , bc , cd , &c, and let the lines bk , cl , dm , en , &c, be drawn parallel to $a\alpha$. It is evident that the air contained between bk and cl , is rarer than the air contained between $a\alpha$ and bk , the former having a lesser column of air $xclx$ incumbent upon it than the column $xbkx$, which presses upon the latter. Upon the same account the air between cl and dm is rarer than that between bk and cl , and that between dm and en rarer than that between cl and dm ; and thus every superior inch of air is rarer than that below it.

Let us now suppose that every inch of air is in all parts of it of an equal density, or that the air ak is every where uniform, but denser than the air bl , which is also supposed to be every where uniform, but denser than cm , and that to be uniform itself, but denser than dn , and so onwards.

Again let us suppose that the air bl is reduced to a lesser space bq , so as to become equally dense with the air ak , which is done by making the space bq lesser than bl , in the same proportion that the air bl is less dense than the air ak ; after the same manner let the air cm be reduced to the space cr , and the air dn

to the space ds , and so onwards, that thus every inch of air may be reduced to the same consistence with the air ak .

Now it is evident from this construction, that the spaces ak , bq , cr , ds , &c, will every where be as the densities respectively of the several inches of air, ak , bl , cm , dn ; and it is also evident, that the quantity or weight of the air which reaches from any one of those spaces up to the extremity of the atmosphere, will every where be as the sum of all the spaces which are situated above the space proposed. Thus the quantity or the weight of air above the space ak , will be as the sum of the spaces bq , cr , ds , et , fv , &c, and the quantity or weight of air above the space cr , will be as the sum of the spaces ds , et , fv , &c. For the air being every where reduced to the same consistence, the quantity or weight of it will be as the space it possesses.

These things being laid down I may now without much difficulty proceed to establish the conclusion I am at, which is this; That if any number of distances from the surface of the earth be taken in an arithmetical progression, the densities of the air at those distances will be in a geometrical progression.

For since by the experiments which have this day been made, it appears that the density of the air is always as the force which compresses it, we must conclude that the density of the air at any distance from the surface of the earth, is as the quantity or weight of that part of the atmosphere which is above it. Therefore in our scheme, the densities of the air between aa and bk , bk and cl , cl and dm , &c, are to each other respectively as the quantities of air above aa , bk , cl , &c, up to the extremity of the atmosphere. But we saw before that those densities were as the spaces ak , bq , cr , &c, respectively, and those quantities of air reaching to the extremity of the atmosphere

were

were as the spaces $xb\beta qrstvx$, $xc\gamma rstvx$, $xd\delta stvx$ respectively; it follows then that the spaces ak , bq , cr are to each other respectively as the spaces $xb\beta qrstvx$, $xc\gamma rstvx$, $xd\delta stvx$.

Now the former spaces ak , bq , cr are the differences of the latter, and it is well known to those who understand any thing of the nature of proportions, when any set of quantities are to each other respectively as their differences, that then as well the quantities themselves, as their differences, are in a geometrical progression (*a*).

The spaces ak , bq , cr , are therefore in a geometrical progression, as the distances ab , ac , ad are in an arithmetical progression. And as the densities of the air belonging to these three first inches, are in a geometrical progression, so do the densities of the air belonging to every one of the other inches, which are supposed to be continued up to the extremity of the atmosphere, decrease in the same geometrical progression, as any one without difficulty may collect by the same way of reasoning.

I have hitherto supposed for ease of conception, that the air is of the same density in every part of each inch of altitude; nevertheless it is certain that every the least variation of altitude causes a variation of density in the air. The conclusion however will not hereby be disturbed; for if instead of dividing the altitude of the atmosphere into inches as before, we conceive it now to be divided into its most indefinitely minute parts, applying to these what we have said above concerning the inches, we shall at length deduce the same geometrical progression of densities answering to a like arithmetical progression of altitudes.

(*a*) Suppose $a : a - b :: b : b - c :: c : c - d :: \&c$, then conversely we have $a : b :: b : c :: c : d :: \&c$.

Now because the rarity of any body is reciprocally as its density, we may also conclude that, as the distances from the surface of the earth do increase in an arithmetical progression, so do the different degrees of rarity of the air increase in a geometrical progression.

This property of the air was first, that I know of, observed by Dr. *Halley*, but because his demonstration cannot be understood by those who are unacquainted with the nature of the hyperbolick line, and Dr. *Gregory* in his demonstration of the same thing, which may be seen in the fifth book of his *Astronomy*, supposes his reader to be furnished with so much geometry as not to be ignorant of the properties of the logarithmick line, I have endeavoured to make the thing intelligible by a method which may be easy even to those who have never medled with curvilinear figures.

Let us see now what help we have from this property, to determine how much the air is really rarefied at any proposed elevation from the surface of the earth.

Since the elevations are the terms of an arithmetical progression as the rarities are the terms of a geometrical, it follows that the elevation is every where proportionable to the logarithm of the rarity. If then by experiment we can possibly find the rarity of the air at any one elevation, we may by the rule of proportion find what is the rarity at any other proposed elevation: by saying, as the elevation at which the experiment was made, is to the elevation proposed, so is the logarithm of the air's rarity which was observed at the elevation where the experiment was made, to the logarithm of the air's rarity at the elevation proposed.

Thus I collected from the celebrated French experiment at the *Puy de Domme*, which I yesterday gave
you

you an account of, that at the altitude of 7 miles the air is somewhat above 4 times rarer than at the surface of the earth. By the same method I collected from the experiment of Mr. *Caswell*, made upon Snowden Hill, that at the same altitude of 7 miles the air is not altogether so much as 4 times rarer than at the surface; the difference on both sides was inconsiderable. We may take a mean therefore and say in a round number, that at the altitude of 7 miles the air is about 4 times rarer than at the surface of the earth.

Sir *Isaac Newton* in his late additions to his *Opticks*, makes use of this very proportion (*b*), what grounds he went upon is difficult to guess, however I am satisfied of the conclusion from my own computation. Now from what has been already proved, that the rarity of the air is augmented in a geometrical, as the altitude is augmented in an arithmetical progression, it follows that every seven miles added to the altitude, does always require a rarity of the air still 4 times greater. Therefore at the altitude of 14 miles the air is 16 times rarer than at the surface, at the altitude of 21 miles it is 64 times rarer, at the altitude of 28 miles 256 times, at 35 miles 1024 times, at 70 miles about a million of times, at 140 miles a million of million of times, at 210 miles a million of million of millions of times, if the air can possibly expand itself to so large dimensions.

Hence we may easily gather that the air at the altitude of 500 miles, if the atmosphere can reach so far, must necessarily be there so much rarefied, that if a globe of the air we breath in, of an inch diameter, were as much dilated, it would possess a larger space than the whole sphere of Saturn. The semidiameter of the earth is nearly 4000 miles, which is 8 times 5

(*b*) In the last edition he makes it 4 times rarer at the height of $7\frac{1}{2}$ miles, and 16 times rarer at 15 miles, and so on, but gives no reason for this alteration.

hundred miles; with good reason then might that excellent philosopher I have lately been mentioning, tell us in his *Principia*, that the air at the altitude of a semidiameter of the earth, is at least so wonderfully rarefied as I have decribed it to be at an altitude 8 times less.

It appears from the observations of astronomers of the duration of twilight, and of the magnitude of the terrestrial shadow in lunar eclipses, that the effect of the atmosphere to reflect and intercept the light of the sun, is sensible even to the altitude of between 40 and 50 miles. So far then we may be certain that the atmosphere reaches, and at that altitude we may collect, from what has been already said, that the air is about 10000 times rarer than at the surface of the earth. How much farther than this altitude of between 40 and 50 miles the atmosphere may be extended, I must confess I am altogether ignorant, there being no *data*, that I know of, from which a greater altitude may indubitably be concluded.

There has indeed been often seen in the atmosphere some very luminous parts, even near the zenith about midnight, but I dare not conclude any thing from such appearances. If I should assert, as some have done, that these luminous parts are nothing else but some terrestrial exhalations floating in the air at a prodigious altitude, and thereby reflecting the light of the sun, which they are exposed to at that great height, to our eyes, it will be next to impossible to give any tolerable account, how those exhalations can be dense enough to reflect so copious a light at that vast distance, and at the same time be supported by a medium, I may say, almost infinitely rarer than the air we breath in. It seems more probable that these extraordinary lights proceed from some self-shining substance or aerial phosphorus.

A surprising appearance of this kind was seen here at Cambridge about 10 of clock at night, and at other very distant places, on the 20th of march in the year 1706. It was a semicircle of light, of about two thirds of the ordinary breadth of the milky way, but much brighter. The top of it passed very near our zenith inclining about 4 or 5 degrees to the north; it crossed the horizon at a very small distance from the west towards the south, and again about as far from the east towards the north. It was most vivid and best defined about the western horizon, and most faint about the zenith, where it first began to disappear: there was at the same time an *Aurora borealis*. A friend of mine saw the same appearance in Lincolnshire, at the distance of about 70 miles north of Cambridge; the semicircle seemed to him to lie in the plane of the æquator. From these two observations compared together it is easy to collect, that the matter from which that light proceeded, was elevated above the earth's surface between 40 and 50 miles.

Having now finished what I design'd to represent concerning the limits and different degrees of rarity of the atmosphere at different altitudes, I might here conclude; but because it may possibly be expected I should add something in this place concerning the cause of the air's elasticity, upon which these deductions were grounded, it may not be amiss to declare here, that of all the several hypotheses which have hitherto been suggested for this purpose, that of Sir *Isaac Newton* seems to me to be the most probable. He has demonstrated in the second book of his *Principia*, that if the particles of the air be of such a nature as to recede from each other with centrifugal forces reciprocally proportionable to their distances, they will compose an elastical fluid whose density will always be as the force which compresses it; and any one
who

who reads the late additions to his Opticks will perceive that that hypothesis is not advanced without reason.

LECTURE X.

The effects of the weight and spring of the air in syringes, pumps, syphons, polished plates, cupping-glasses, suction, respiration, &c.

I HAVE hitherto been proving that the air has weight, and consequently presses upon all bodies to which it is contiguous. We have found that at the surface of the earth, the pressure of any column of air is equivalent to the weight of a column of quicksilver having the same basis and its altitude of about $29\frac{1}{2}$ inches; or to the weight of a column of water, having the same basis and its altitude about 34 feet; that the pressure is lessened always as the elevation from the surface of the earth becomes greater; that the air has also an elastical power, by which it endeavours as far as is possible to expand itself; that this elastical power of the air is equal to the force which compresses it; that the space it possesses is always reciprocally as that force, and consequently its density directly as the same; that the degrees of density of the atmosphere are different at different altitudes, the air being still rarer as the altitude is greater; that the rarity of the air increases in a geometrical progression as the altitudes increase in an arithmetical one, the air at every 7 miles of height being always 4 times more rare than before.

Let us now come to those effects of the pressure of this subtle fluid, whether caused by its weight or spring, which were formerly thought to proceed from that abhorrence which they say nature ever has of a *vacuum*. Amongst these we may reckon the phenomena of syringes, pumps, syphons, polished plates, cupping-

cupping glasses, suction, respiration and others of the like nature. Mr. *Pascal* in his little French treatise concerning the gravity of the air, has given us a very good account of these things. I shall therefore for the most part make use of his explications, since it would be needless to go about to make new ones. His method is this, he first recites the principal effects which were wont to be ascribed to a *fuga vacui*, and afterwards shews that they proceed from the pressure of the air.

First then a pair of bellows whose vents are all well closed up, are difficult to be opened; as we attempt to do it we perceive a resistance as if the sides were glued together. After the same manner the sucker of a syringe, which is stopped at the bottom, resists the force we apply to draw it out, as if it were some way fastened to the bottom. It is pretended that this resistance proceeds from the abhorrence which nature has of a *vacuum*, which would happen in both cases, if the sides of the bellows were disjoined, or the sucker of the syringe drawn out. That opinion is confirmed by this, that the resistance ceases as soon as the air is permitted to enter.

Secondly two polished bodies applied together are difficult to be separated and seem to adhere to each other. It is pretended that this adherence proceeds from the like abhorrence of a *vacuum*, which would happen during the time which the air would take up in coming from the edges to the middle.

Thirdly when the pipe of a syringe is immersed in a vessel of water, if you draw up the sucker the water will follow it and ascend as if it did adhere to it. Thus in a pump which is a longer syringe, the water ascends and follows the sucker, being raised up in the same manner. It is pretended that this elevation of water proceeds from the endeavours of nature against a *vacuum*, which would happen in the space
deserted

deserted by the sucker, if the water should not ascend, since the air is excluded ; which is confirmed by this, that the water will no longer ascend if the engine has any leaks so as to admit the air to come in.

After the same manner if you place the nose of a pair of bellows under water, and open it suddenly, the water will ascend to fill it, because the air cannot, and the experiment will succeed the better if the bellows be entirely closed up. Thus placing your mouth under water and sucking, you may attract the water for the same reason ; for the lungs may be compared to a pair of bellows. Thus in respiration we draw in the air, just as a pair of bellows by being opened attracts the air to fill up its cavity. Thus if you place a lighted piece of paper in a glass, and suddenly invert it into a vessel of water, as the flame decreases so will you see the water ascend into the glass ; for the air in the glass being rarefied by the flame, when it afterwards comes to be condensed by the cold water, upon contracting its dimensions it will draw up with it some of the water to fill the space it has deserted. Thus do cupping glasses draw the flesh and cause a swelling ; for the air in the cupping glass being rarefied by heat, when it comes again to be condensed after the flame is extinguished, it draws in the flesh to fill up the space it has deserted, as before it drew in the water.

Fourthly if you fill a bottle with water, and invert the neck of it into a vessel filled with other water, the water will remain suspended in the bottle without falling out. It is pretended that this suspension proceeds from a *fuga vacui* ; for there would necessarily be left a void space if the water should descend, since the air cannot come in to fill it up ; which they confirm by this observation, that if the air be suffered to enter by some hole the water will immediately fall down.

Fifthly if syphon be filled with water and its
1
legs

legs be immersed into two different vessels of water, it will come to pass, if one of the vessels be higher than the other, that the water contained in the higher will ascend to the top of the syphon, and then descend into the lower vessel, so that if you continually supply the higher vessel with water, the flux will be perpetual. It is pretended that this elevation of water is to be ascribed to the endeavours of nature to hinder a *vacuum*, which would happen within the syphon, if the water contained in those two legs should descend each into its vessel below; which actually comes to pass when the air can come in at the top of the syphon through some hole.

Many other effects there are of the like nature, which have been omitted as being nearly the same with those already described; in all of them there appears nothing more than this, that all contiguous bodies resist any effort made to separate them, when the air cannot succeed them; whether that effort be their own proper weight, as in the examples where water ascends and remains suspended notwithstanding its weight; or whether it proceed from some force applied to disunite them, as in the first example. Such effects as these have commonly been ascribed to a *fuga vacui*, let us now see how they depend upon the pressure of the air.

To explain how the pressure of the air is the cause of that difficulty we perceive in opening a pair of bellows, whilst the air has no ingress, Mr. *Pascal* puts his reader in mind of what he had before been discoursing of, in his other treatise, concerning the *equilibrium* of liquors, that if a pair of bellows whose pipe is 20 feet long or more be placed in a deep vessel filled with water, so that the end of the pipe be above the surface of the water, it will be very difficult to be opened, and by so much the more as the altitude of the water above the sides of the bellows is

is the greater ; which proceeds manifestly from the weight of the superior water. For before any water be poured into the vessel, there is no difficulty in opening the bellows, but as more and more water is poured in, the resistance is continually augmented, and is always equivalent to the weight of the water which is supported. For as no water can enter into the cavity of the bellows, the orifice of the pipe being above the surface, it is evident that the sides cannot be disjoined without raising and sustaining the superior mass of water. Now no body can here say that this resistance proceeds from a *fuga vacui*, since the air has a free passage into the cavity by the orifice of the pipe which is above the water ; it is absolutely certain therefore, that it depends intirely upon the weight of the water.

What has been here said as to the weight of the water, may be applied to any other fluid ; for if the bellows be placed in a vessel filled with wine, there will be the like resistance, and the same may be said as to milk, oyl, quicksilver, or any other fluid whatever. It is then a general rule and a necessary effect of the gravitation of fluids, that if a pair of bellows be placed in any fluid whatever, so that the fluid have no access to the cavity of the bellows, the weight of the superior parts of the fluid will cause a resistance to the opening of the bellows. If therefore we apply the general rule to the air in particular, we may say that when a pair of bellows is so stopt as to leave no ingress to the air, the weight of the superior mass of air will cause a resistance in opening the bellows, which resistance will cease as soon as the air is permitted to enter.

What has been said as to this effect will hold good as to others, in which I may be the more succinct, having enlarged already so much upon this. It has been already shewn in the former week that the pressure

sure of any fluid may produce effects analogous to those of the syringe, pump, syphon and polished plates, and the application was at the same time made to the air; I need not here therefore insist upon them any longer. That would also have been the proper place for this experiment of the bellows under water, could it as easily have been made as described. However the description of it may serve at least to illustrate the conclusion upon whose account it was proposed by its ingenious author. For it cannot but be evident to any one who is at all acquainted with hydrostaticks, that the event must needs answer the description that has been given of it.

The same thing also may be said of the following experiment or instance proposed by the same person to illustrate the effect of cupping glasses. He supposes that a tube of about 20 feet in length open at both ends, has one end which enlarges itself like the mouth of a funnel, applied to a man's thigh at a considerable depth under water, so as to hinder the water from pressing upon that part alone of the thigh which is included within the orifice of the tube, to which the air nevertheless has a free access by the other end of the tube which is above the surface of the water. In this case, says he, it will come to pass, that the part included within the orifice of the upper tube, will be considerably swelled out as if something sucked it in that place. Now it is plain that this swelling can by no means be said to proceed from a *fuga vacui*, since the tube is open to the air, and no such thing would happen if there were not any, or but very little, water to press upon the rest of the body. It is therefore most certain that this effect depends purely upon the gravitation of the water; for whilst it presses upon all other parts of the body excepting that alone which is covered by the tube, to which it has no access, it forces the blood and other yielding parts to arise where
there

there is not so great a pressure, and thereby causes the swelling.

What has been said as to the pressure of water will hold true as to the pressure of any other fluid; and therefore the pressure of the air may cause a like swelling, if it be greater upon the other parts of the body than upon that to which the cupping glass is applied, as it certainly is. For the air within the cupping-glass being very much rarefied, and consequently in part expelled by the heat, when it comes again to its usual temper, its spring will be very much debilitated, and therefore it will press less forcibly against the part of the body under the glass, than the external air upon the other parts of the body.

I need not now use many words to explain how it comes to pass, when any one places his mouth under water and sucks, that the water ascends; for it is clear that the external air presses upon every part of the surface of the water, excepting that which is covered by the mouth; and hence it happens, that when the muscles serving for respiration, elevate the ribs and enlarge the capacity of the chest, the air within having a greater space to fill than before, hath less force to hinder the entrance of the water into the mouth, than the external air has to promote that entrance. This also is the cause of the attraction or suction of any liquor by a tube, and it differs very little from the effect of the syringe. Thus a sucking child at the breast of its nurse, draws in its milk; the external air pressing the breasts of the nurse on all parts excepting that which is covered by the child's mouth. Upon the same account in respiration the air enters into the lungs; for as the chest is dilated, so is the external air forced in by the weight of the superior part of the atmosphere; which is so intelligible, so easy and natural, that one would wonder that philosophers should ever have had recourse to a *fuga vacui*.

vui, to occult qualities, to causes so foreign and chimerical.

Thus may all the other effects which were once ascribed to a *fuga vacui*, be shewed to depend upon the pressure of the air, as cannot but be evident to those who understand the principles of hydrostaticks, and are satisfied that the air is a gravitating fluid; which thing I hope I have already proved, and shall hereafter further confirm. But as the weight of the air is not infinite, but limited by certain bounds, so are the effects depending thereupon also limited. Thus water cannot by a pump be raised to any proposed altitude. We know that a column of water of about 34 feet in the altitude, is commonly a counterpoise to the pressure of the atmosphere; that therefore is the utmost height to which the air in its mean state of gravity can elevate the water in a pump. If the air happens to be more than ordinarily heavy, the water will ascend something higher, but scarce ever more than 36 feet. If the air be more than ordinarily light, the water will not come up 34 feet, nevertheless the air is seldom so light as not to be able to bear up water so far as 32 feet.

If the operation of the pump did depend upon the *fuga vacui*, as it was commonly believed before Galilæo's time, then it would follow that water might be raised to any altitude how great soever; for why should not nature have as great an aversion against a *vacuum* in one case as in another? And accordingly several who embraced that notion, have very confidently asserted, though they never made the experiment, that it might be raised *ad libitum*. But Galilæo observing that there was a certain standard altitude, beyond which no water could be elevated by pumping, took an occasion from thence to call in question the doctrine of the schools concerning the *fuga*, which began from that time to be very much suspected, and

in the room thereof he happily substituted the hypothesis of the air's pressure and gravitation. It was to him indeed little better than an hypothesis, since it had not then those confirmations from experiments which were afterwards found out by his scholar *Torricellius* and other succeeding philosophers, particularly our excellent Mr. *Boyle*.

What has been said of pumps may be also applied to syphons. It was formerly looked upon as unquestionable, that water might be conveyed over the highest mountains by the help of this instrument, if the place into which it was to be discharged, were but lower than the place from whence it was derived. We are now certain of the contrary by experiments made more than once: 34 feet is commonly the utmost height to which water can rise as well in syphons as in pumps. In quicksilver the utmost altitude is less, being commonly about $29\frac{1}{2}$ inches, $29\frac{1}{2}$ inches of quicksilver and 34 feet of water being a counterpoise to the pressure of the atmosphere, upon which those effects depend.

I will add but one instance more concerning polished plates; as their cohesion depends upon the limited pressure of the air, so is the force requisite for their separation also limited, and may be thus computed. Since the force requisite for their separation must be equal at least to the force which causes their cohesion, that is, to the pressure of the air, and the pressure of air upon any basis is equal to the weight of a column of quicksilver having the same basis and the altitude of about $29\frac{1}{2}$ inches, it follows that the force requisite to separate the plates, ought to be equal at least to the weight of a cylinder of $29\frac{1}{2}$ inches altitude, having the area of the plates for its basis. By calculating upon these grounds, I find that the force requisite to separate our larger marble plates, is equal to about one hundred weight and $\frac{3}{4}$; and the force requisite

site to separate the lesser brass plates amounts nearly to 73 pounds weight; and this upon supposition that they are perfectly well polished and so fitted together that no air can intervene. But as they want of that perfection, so will a lesser force be sufficient to disjoin them.

LECTURE XI.

The phænomena of capillary tubes, glass planes, the figures of the surfaces of fluids and other things relating to the same head, considered.

WE are now upon a subject abounding with difficulties, which has in vain been attempted by several modern philosophers. Many hypotheses they have invented to account for these odd appearances, which if thoroughly examined will be found to be but bare hypotheses, and in many particulars insufficient. It has been generally believed, that the unequal pressure of the air upon the liquor contained in the tube and that in the vessel, is the cause of the ascent in the tube. For if the pressure upon the liquor in the tube be less than that upon the vessel, the liquor ought to ascend so far in the tube, that its own weight together with the weaker effort of the air incumbent upon it, be equal to the free and unrestrained gravitation of the atmosphere upon the vessel.

Though they have generally agreed in this, that there is a lesser pressure of air upon the liquor in the tube than upon that contained in the vessel, yet the causes they have assigned for that inequality are very different. Some have had recourse to the magnitude of the particles of the air and of the ascending fluid. Others have believed, that only an inverted cone of air, touching the surface of the liquor in the tube with its vertex, and having the upper orifice of the tube

for its basis, could press upon the surface contiguous to its vertex. Dr. *Hook* supposes part of the pressure of the air in the tube, to be taken off by its friction, which he supposes must necessarily happen against the sides in so narrow a passage. Other conceits there are which I have omitted, being more concerned to find out, if I can, what is the truth, then to enumerate the groundless fancies of those who seem to me to have missed of it.

Dr. *Hook*'s hypothesis has indeed the fairest shew of probability, and accordingly it has been received with great applause. It may therefore be worth while to give an account of it, and to examine afterwards whether it be such as we may acquiesce in. That there is an inequality of pressure he endeavours to make out from hence, that there is a much greater incongruity of air to glass and some other bodies than there is of water to the same. By congruity he means a property of the fluid body, whereby any part of it is readily united with any other part, either of itself or of any similar fluid or solid body; and by incongruity, a property by which bodies are hindered from uniting with any dissimilar body. Thus, not to mention several chymical spirits and oyls, which will very hardly, if at all, be brought to mix with one another, if we observe the drops of rain falling through the air, and the bubbles of air which are by any means conveyed under water, or a drop of fallad-oil swimming upon the water, we cannot be to seek for instances of the incongruity of fluids amongst one another. And as for the congruity or incongruity of liquids with several kinds of firm bodies, they have long since been taken notice of and called by the names of dryness and moisture; though these two names are not comprehensive enough, being commonly used to signify only the adhering or not adhering of water to solid bodies. Thus we may observe that water will
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more readily wet some woods than others, that water let fall upon a feather, the whiter side of a colewort and some other leaves, or almost upon any dusty, unctuous or resinous surface, will not adhere but easily tumble off from them, like a solid bowl; whereas if dropt upon linnen, paper, clay, green wood, &c. it will not go off without leaving some part of itself behind. So quicksilver, which will very hardly be brought to stick to any vegetable, will readily adhere to and mingle with several clean metalline bodies.

The cause which he proposes of this congruity and incongruity of bodies is, that all fluids are in a sort of vibrative motion, which he says is a sort of pulse or shake of heat, by which the parts of bodies being made loose from one another, can easily move any way and are thereupon fluid. If in a large dish several kinds of sands be mixed together, we shall find that by any vehement agitation, the fine sand will eject and throw out of itself all the bigger bulks of small stones and the like, which will be gathered together into one place; and if there be other bodies in it of other natures, they also will be separated into a place by themselves. In like manner he supposes the pulse of heat to agitate the small particles of matter, and those which are of the same bigness, figure and texture will hold or dance together, and those which are of a different kind will be thrust out from between them by several variations of harmony and discord. And what has been said as to fluids he supposes may be attributed to solid bodies, to which also he applies the like vibrative motion. This is his explication of congruity and incongruity.

If then we allow, as we easily may, that water is more congruous to glass than air is, it will follow that water may more easily be forced through the narrow passage of a slender pipe than air. He illustrates

the thing by the resemblance of a round spring, such as an hoop: for as in a round spring there is required an additional pressure against the two opposite sides to reduce it into an oval form, or to force it in between the sides of an hole whose diameter is less than that of the spring, so to alter the spherical constitution of the air included in the tube, arising from its incongruity to glass, there is required more pressure against the opposite sides to reduce it to an oval; and to press it into a hole less in diameter than itself, it requires a greater protrusion against all the other sides, which he found also to be true by experiments. Therefore he concluded, that part of the pressure of the atmosphere being taken off and spent in protruding the air within the cavity of the slender tube, it has less force to resist the ascent of the water, which is impelled upwards by the whole force of the atmosphere, pressing upon the surface of the water in the vessel into which the lower end of the tube is immersed; and as a greater part of the pressure of that column of the atmosphere, which is incumbent over the upper orifice of the tube, is taken off in protruding the air within the tube when it is slenderer, so is its resistance to the ascending liquor still less, and consequently the ascent is greater.

This is Dr. *Hook*'s account of the matter, which any one may more fully acquaint himself with, by reading the sixth observation of his admirable *Micrography*. It appears at first view to be very satisfactory, and accordingly it has not wanted its patrons; nevertheless I must here confess that I cannot by any means persuade myself to be of the same opinion. Amongst many others, this is one great reason of my backwardness to embrace Dr. *Hook*'s hypothesis, that I have found by making the experiment, and several others have took notice of the same thing before me, that even under a receiver exhausted of air by the

the air-pump, there is as far as we can perceive a like and equal ascent of liquors in capillary tubes. Now if the difference of the pressure of the air upon the liquor contained within the tube and upon that within the vessel, into which the tube is immersed, be the true cause of the ascent, we must needs be at a loss in explaining how the same ascent can possibly happen in *vacuo*, when there is no air to press either upon the liquor in the vessel or that in the tube.

It may perhaps be answered, that we can never by pumping perfectly evacuate the receiver of air, so that after all our endeavours there will still a sufficient quantity remain behind to produce the effect: let it then be considered whether that very small quantity which remains behind, can possibly be said to be sufficient. It is reasonable to conclude, that as the quantity of air under the receiver is diminished, so are the pressures of the air upon the liquor in the vessel and upon that within the tube proportionably diminished, and consequently the difference of those pressures also. If then that difference be the cause, as is pretended, of the ascent of the liquor in the tube, as that difference is diminished by pumping, so ought the ascent in like manner to be diminished, which does not happen; therefore that difference is not the cause of the ascent. If it be said, contrary to all reason, that the difference of those two pressures is not diminished as the receiver is evacuated, I might easily prove, were it necessary, that even the whole pressure of the air in the receiver upon the liquor in the vessel, is not able to bear up the liquor in the tube to the height it will ascend to, if the bore be narrow, when the pump is sufficiently worked: much less then can the excess of that whole pressure upon the liquor in the vessel above the pressure upon the liquor within the tube, be said to do so, unless the part can be proved to be greater than the whole. We have therefore good grounds from the abovementioned e-

vent of the experiment made in the evacuated receiver, of which you will hereafter be eye-witnesses, to distrust that cause arising from the inequality of the air's pressure.

It will follow therefore, that there is no such inequality of pressure as is pretended; for if there were, then would the ascent in the open free air be greater than in *vacuo*, the inequality of pressure co-operating in the open air with the cause of the ascent in *vacuo*, whatever it be.

But it may not be amiss in this place to shew a *priori* also, that there is not that inequality of pressure, notwithstanding that there be required a considerable force to protrude the air contained in the tube. The pressure of the incumbent part of the atmosphere endeavours to force the included air downwards, and the pressure of the elevated liquor does at the same time endeavour to force it upwards; now these two forces are in *equilibrio*, otherwise the included air would be more protruded downwards or more upwards, till that *equilibrium* were gained. We must therefore necessarily conclude that the pressure of the elevated liquor upwards, is equivalent to the whole weight of the incumbent part of the atmosphere. Therefore it is pressed downwards, by the air which is contiguous to it, with a force equivalent to the weight of the incumbent part of the atmosphere; for were it kept down by a less force than that with which it endeavours to ascend, it would ascend further. Every equal part of the surface of the liquor in the vessel below, is also pressed downwards by the same whole weight of the incumbent part of the atmosphere. The liquor contained in the tube is therefore exposed to an equal pressure with that which is contained in the vessel; that fancied inequality cannot therefore take place. I have proved both a *posteriori* and also a *priori*, that the inequality of the air's pressure is not the cause of the ascent of liquors in capillary tubes.

I may add further that the difficulty of protruding the air through the narrow passage of the tube, is so far from being the cause of the liquor's ascent, that it would rather hinder that effect than promote it. *Dr. Hook* indeed from thence might with some probability have given an account why an elevated liquor should not be depressed, but the spontaneous ascent of liquors does manifestly contradict his opinion, since the difficulty the air has to pass upwards along the tube, when urged by the ascending liquor, ought more to resist its ascent than the free and unrestrained pressure of the atmosphere in a tube much larger.

Whether his hypothesis concerning the congruity and incongruity of bodies, be such as we may securely admit of, I will not stay here to examine. I confess I see not any necessity of supposing the particles of fluids or firm bodies to be perpetually either in a vibrative or any other motion. It may so happen, that by accidental motions of the air or other contiguous bodies, the parts of fluids may seldom be at rest; but that a perpetual intestine motion is essential to fluids, is what has not yet, that I know of, been demonstrated, though some have attempted it.

I have hitherto been only proving what is not the cause of the effects we have been considering. Let us now try whether we can find out what is the true cause. It is a common observation, that a drop of water of a certain determinate magnitude, will firmly adhere to the surface of glass and of other bodies, and even hang pendulous to it, though the surface be placed downwards, notwithstanding the weight of the drop which endeavours to disjoin it. Since then that endeavour is rendered some way or other ineffectual, we need not scruple to ascribe a mutual attraction to the glass and water: it is evident that congruity alone, in *Dr. Hook's* sense, is not sufficient to hinder

hinder the drop from falling by the force of its own weight; there is something more required than a bare congruity to overcome that force.

What I call attraction, any one if he thinks fit may give another name to; I mean no more by attraction than some power in nature, from what causes soever it proceeds, by which bodies do endeavour to be united to one another. This adhesion of the drop to the surface of the glass and other bodies, has commonly been ascribed to the pressure of the air. We have seen that this pressure is able to keep well polished plates united, notwithstanding a considerable force was used to separate them; but then the air must be excluded from between the surfaces. Now a drop or globule of water will be attracted to a plate of glass, which touches its upper part, notwithstanding the intervention of the air immediately before the attraction; which shews that the case is different from that of polished plates. We may also satisfy ourselves that this adhesion does not proceed from the pressure of the air, by the air-pump, since the same will happen under an exhausted receiver.

Certain it is, whatever be the cause of it, which I pretend not to determine, that there is such an attraction between water and other liquors to glass and several other bodies. And as there are attractions between several particular bodies, so we may observe others mutually to repel each other. Thus do the particles of air seem to fly asunder with forces reciprocally proportionable to their distances, and thereby compose an elastical fluid, whose density is as the force which compresses it. After the same manner it is very probable that air endeavours to recede from several dense bodies. Thus also do several other, as well solid as fluid bodies, seem to repel each other, and this is what Dr. *Hook* took notice of in many bodies which he therefore calls incongruous. Instances enough

nough of this kind may be seen in the last query at the end of the last edition of Sir *Isaac Newton's* Opticks, which I forbear to transcribe. Whoever will read those few pages of that excellent book, may find there in my opinion, more solid foundations for the advancement of natural philosophy, than in all the volumes that have hitherto been published upon that subject.

But, to proceed with our drop of water, we may observe that the force of the attraction I have been speaking of, is of a certain determinate quantity. If the drop be too big, it will fall off, the force of its own weight being greater to separate it, than the force of attraction to hinder that separation. As the part of the surface of the glass, to which the drop is contiguous, is larger, so will it bear up a greater drop; a larger surface having proportionably a greater attraction.

It is easy to apply what has been said concerning a plane surface, to the inner concave surface of a narrow cylindrical tube. It cannot but be evident, that this ought, as well as the other, to attract and hold up a certain weight of water within the tube. The attracting surface does in this case every way surround the drop, and by that means has a much greater advantage to bear it up, than if it were a plane, and could thereby touch it only in one of its sides. Now as the diameter of the tube becomes lesser, so is this advantage still increased; for it is well known that the surfaces of cylinders bear a greater proportion to their capacities, as their diameters are more and more diminished, the surfaces decreasing only in the same proportion with the diameters, and their capacities decreasing in a proportion which is duplicate of that of the diameters. Hence it comes to pass that the water can be held up at a greater altitude as the tube is narrower, which is the thing we were chiefly concerned to account for. It

It is plain from these principles, that the event ought to be the same in *vacuo* and in the open air; that the same quantity of liquor ought to be suspended in the tube when taken out of the vessel, as was before elevated above the surface of the liquor contained in the vessel; that, if when the tube is taken out of the vessel, a piece of glass be applied to its under orifice, so as to touch the suspended liquor, by attraction it will cause it to descend out of the tube. The experiments of capillary siphons are explicable upon the same grounds. The liquor ascends to the top of the flexure by the attraction I have been speaking of, and then by its own weight it descends along the other leg. It would be tedious to instance in more particulars; the application of what has been said to other cases, is so easy that no body can miss of it.

The reason of the different figures of the surfaces of fluids, is very obvious and depends upon the same principles. Water forms itself into a concave, the superficial parts of it, which are near the sides of the tube, being attracted upwards to the glass. Quicksilver forms itself into a convex, being repelled from the glass (*a*), which repulsion is also the cause why it does not, as water, ascend above the level in capillary glass-tubes, but on the contrary remains below it. If two liquors be placed in the same tube contiguous to each other, and both be equally attracted to the sides of the tube, their common surface will be a plane. If one be somewhat more attracted than the other, that which is most attracted will have a concave surface, and the other which is less attracted,

(*a*) This repulsion is not real but only apparent and relative. For Dr. *Furin* has plainly shewn, that glass attracts the particles of quicksilver, but not so strongly as they attract one another; and upon this principle has clearly explained the phænomena of quicksilver in capillary tubes and between glass planes. *Phil. Trans.* N^o. 363.

must of consequence have a convex, the surfaces of the two liquors being contiguous; and thus a liquor whose surface is concave when exposed to the air, may have that surface changed into a convex, by the contact of another liquor which is more powerfully attracted to the sides of the containing vessel than itself.

It is not difficult to understand that the experiments we made concerning the motions of floating bodies are deducible from the like causes. If any one would be more particularly informed about this matter, he may consult Mr. *Marriotte's* *Traité du mouvement des eaux*, or the fourth volume of *Du Hamel's* *Burgundian philosophy*.

From what has been said concerning the ascent of liquors in capillary tubes, we may easily understand how filtrations of all sorts are performed. If a tube be filled with sand or sifted ashes well pressed together, and one end of it be placed in a vessel of water, the water will be attracted by the sand or ashes, and rise to a great height above the level of that within the vessel. Thus if any part of a piece of cap-paper, or a sponge, or a piece of bread or sugar, or of linnen, or of several other substances, be wetted, the moisture will be propagated to the other parts by the power of attraction.

This is the cause of the ascent of spirit of wine, oyl, melted tallow and other unctuous bodies, into the wick of a lamp or candle. It is very reasonable to believe that this is also the cause of the ascent of the sap in trees, and of the various secretions of fluids through the glands of animals, and of several other effects in nature, which any thinking person cannot miss of.

LECTURE XII.

The air-pump and instruments for condensing and transferring air; their fabrick, operation and gages explained.

HAVING in the first week of this course by several deductions and conclusions from experiments, which we thought to be the most pertinent, endeavoured to establish the true and genuine principles of hydrostaticks, and to demonstrate the most fundamental properties of fluid bodies in general, we proceeded in the second week to a more particular consideration of the air; a fluid contrived by the wise Author of nature for so many various, admirable and excellent ends and purposes, and manifestly fitted to have so universal and useful an influence upon the whole system of bodies we are particularly concerned with, that it very much deserves our utmost diligence and most careful examination. It has been already proved, I think beyond any reasonable contradiction, that this subtle element is by no means privileged or exempted from that catholick law of gravitation, to which (as far as appears from observations that have hitherto been made by inquisitive philosophers) all matter is alike and equally subject, of what form or texture soever it be; and it is upon the account of this its ponderousness and its fluidity that it is qualified to exhibit all the various appearances of other fluid and heavy bodies, as has been made manifest in several instances, when we compared the remarkable phænomena of the Torricellian tube, of pumps, syringes, syphons, polished plates and some other effects of the like nature, with the more common and obvious, and therefore less surprising, effects of grosser and more sensible fluids, such as water and quicksilver. We

We have found moreover that the air is endowed with a very considerable power of elasticity or springiness, by which it perpetually endeavours to expand itself into larger dimensions, and to remove the obstacles, whatever they be, which confine it to the bounds in which it happens at any time to be contained. We have seen also that it exerts this power the more forcibly as it is the more closely imprisoned and crowded together; and, as if it were desirous of its liberty in the same measure in which it wants it, experience has shewed us that the force it employs to regain its freedom, I mean its elasticity, is ever proportionable to its coarctation or density.

From hence was deduced a method of determining its rarefactions in the several regions above the surface of the earth. You may remember it was proved that as its altitude increased by equal intervals or in an arithmetical progression, so the degrees of rarity were augmented in a geometrical progression. These affections of the air and some others, which I need not now recall to your memory, have been already made out in the preceding week. But the proofs we made use of, though they are very well fitted to satisfy and convince those who are able to give them an attentive and impartial consideration, yet are they of such a nature as to afford some little scope for the petty cavils and exceptions of some philosophers, whose former prejudices had made it seem their interest to oppose them; and being rather the remote deductions of reason than the immediate impressions of sense, they may have, upon that score, the less weight and moment to determine the assent of men who are not much accustomed to abstracted speculations, but are generally to be wrought upon and convinced by motives of a more sensible kind, and less different from the vulgar apprehensions they frame to themselves of the natural appearances of things.

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We shall now proceed in the remaining part of our course to another set of tryals, which will not be so liable to the abovementioned objection. These carrying a more sensible evidence along with them, are for the most part, sufficient of themselves to procure our assent and intire conviction without any further reinforcements, or the assistance of remote inferences to bring them home to our understanding. For which reason, that I may not give you or myself any unnecessary trouble, or mispend our time to no useful purpose, I shall endeavour to avoid all needless prolixity in my future lectures, and shall oftentimes also omit to read any, when either the matter has already been formerly treated of, or is of itself so evident as not to require any further illustration, or lastly, which I confess will sometimes happen, is of so difficult a nature that I cannot pretend to satisfy myself as to the true causes of such surprising appearances; and I am not willing to go about to amuse you with conjectures and seeming probabilities or plausibly contrived hypotheses.

It is not long ago since this method was very much in vogue, but we have seen it of late give way to a sounder and surer manner of philosophizing. It is no very difficult matter for ingenious men, who have sufficient leisure, to frame to themselves such principles of nature as may serve to explain any particular appearance whatsoever. But then the theories which are thus advanced, ought to be looked upon only as philosophical romances, and the witty fictions of inventive brains, unless the truth of those principles and their real existence can be demonstrated, and put beyond dispute by proper experiments. Where this cannot easily be done, it is the safest way, if we are desirous to be free from error and prejudice, to wait till some further light may be afforded us by future observations. The mind of man indeed is naturally desirous of something to rest upon, something in which
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it may acquiesce and on which it may terminate its view ; it is a sort of pain to it to be held long in suspense, and therefore we are willing to take up with any fair shew of an hypothesis, rather than continue, as we are apt to imagine, in a greater degree of uncertainty. But we ought to consider (besides that these hasty and ill grounded conclusions argue a certain weakness and levity in us) that by thus greedily catching at the shadow we commonly lose the substance ; nothing being so great an obstacle to the reception of truth, when it comes to be proposed, as these darling phantoms which use and custom does at length persuade us to be realities. Thus from the time that the philosophy of *Des Cartes* first appeared, the existence of his *materia subtilis* has been looked upon as a thing not to be questioned ; the celebrated feats and wonderful operations of it, have in a manner intoxicated the minds of men, and possessed them with a sort of madness ; insomuch that any attempts which have been made against it, have been thought to be of the most dangerous consequence, as tending to subvert the very foundations of all science. Even *Hu- genius* himself, that great master of reasoning, as he has upon other occasions shewed himself to be, was drawn aside from pursuing better things, by the fondness he had entertained for this principle ; which by a sort of legerdemain could so easily be applied to the solution of the most intricate and perplexing difficulties of nature. One might reasonably have expected that this great man, who appears to have been of a very candid and ingenuous temper, after he had seen and considered the incomparable *Principia* of Sir *Isaac Newton*, and in particular the application of them, in accounting for the heavenly motions to such a wonderful degree of exactness ; and so full and clear a demonstration of the insufficiency of the Cartesian vortices, one might have expected, I say, that after

this further information, he would not have been averse to have altered his sentiments. Notwithstanding this, though he expresses an extraordinary pleasure and satisfaction upon the reading of that admirable book (which he says he looks upon as a surprising instance of the great strength and capacity to which it is possible for the mind of man to arrive) yet we see he could not willingly change his own principles for others; and it was impossible for him to forsake an hypothesis which he had himself very much cultivated, and was so long accustomed to. So great is the force of prejudice that an ill grounded opinion shall often prevail, by long prescription, to obstruct the evidence of a well demonstrated certainty. For my part I think it more adviseable to profess our ignorance where the truth is not yet discovered, than to pretend a knowledge which may for ever hinder us from attaining it.

But to come more immediately to the business of this day, the instruments under our present consideration, which we shall chiefly employ in the remaining part of our course, are the air-pump and condenser. By these we are assisted to make a great variety of tryals, concerning the influence and operation of the air under its most different constitutions, from a very great degree of density to an almost infinite rarefaction.

The condenser is an instrument whose invention is so very obvious, that it was impossible it should escape the curiosity of former ages. It has been so very long in use that I cannot pretend to assign its origin.

The air-pump was first, that I know of, contrived and brought into use by *Otto Guericke* consul of Magdeburg, some time before the year 1654; for then it seems this ingenious gentleman, being employed in a publick negotiation at Ratisbon, had an occasion offered him of shewing his engine to the Emperor and some other princes there present; among whom
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the Elector and Archbishop of Mentz was particularly delighted with the contrivance of the instrument, and the curious experiments exhibited by it; inso-much that he became very desirous of having such another machine made for his own use. But this could not easily be effected by reason of the short stay they had to make at Ratisbon, and for want of skilful workmen. However he prevailed with the inventor to part with his own *apparatus*, and at his return carried it home with him to Wurtzburgh. Here it was that the learned and diligent Jesuit father *Schottus*, being then professor of the mathematicks in that university, had first the sight of it, together with some other curious and learned persons. The archbishop was pleased himself to give them an account of the engine, and a relation of the experiments he had seen the inventor perform at Ratisbon. These they tryed over several times in his presence, and it was not long before they themselves also made several other new ones of the like nature.

The fame of these first essays was quickly spread abroad by the large correspondence which *Schottus* held with learned men in most parts of Europe, but more particularly in the year 1657, when he published his *Mechanica Hydraulico-Pneumatica*; to which as an appendix he added a distinct and full account of these Magdeburgick experiments as he called them. In the year 1664 he published his *Technica curiosa*, and gave a further relation of other new experiments which had been made since the printing of his former book. After this the famous inventor himself *Otto Guericke*, in the year 1672, was pleased to give a most perfect narrative of his own tryals, in his book which he calls *Experimenta nova Magdeburgica de vacuo spatio*. They who are curious to understand the particular fabrick of these first engines, and to observe the gradual improvements which have been

made in these matters abroad, may receive full satisfaction by consulting the books I have been mentioning.

But it is time now that I return to our own countryman, the excellent Mr. *Boyle*, whom I fear I shall be thought to have injured by ascribing these first inventions to a foreigner. The air-pump indeed is so generally known by the name of the *Machina Boyleana*, and the void space produced by it is so commonly called the *vacuum Boyleanum*, that many are thereby persuaded to believe, they owe their original contrivance to this English philosopher. For my part I should rather chuse to give another reason for these appellations, by saying that the engine and void space do very justly bear the name of Mr. *Boyle*, since whoever might happen to be the inventor of them, his certainly was the more excellent part, to have first applied them to such admirable and useful purposes: it being confessed on all hands that the glory of the English experiments, has in a manner totally obscured that of the Magdeburgick.

As to the contrivance of the instrument, he does himself ingenuously confess that it was not his own, in his letter written, two years after *Schottus's* first book was published, to the Lord *Dungarvan*, his nephew, who was then at Paris; in which letter are the following words, which I think not amiss to be repeated to you; that you may the better understand the occasion and manner of his first attempts upon this subject. “ I should immediately proceed, says he, “ to the mention of my experiments, but that I like “ too well the worthy saying of the naturalist *Pliny*, “ *Benignum est & plenum ingenui pudoris, fateri per* “ *quos profeceris*, not to conform to it, by acquaint- “ ing your Lordship, in the first place, with the hint “ I had of the engine I am to entertain you with. “ You may be pleased to remember, that a while be-
“ fore

“ fore our separation in England, I told you of a
“ book that I had heard of, but not perused, pub-
“ lished by the industrious Jesuit *Schottus*, wherein it
“ was said, he related how that ingenious gentleman
“ *Otto Guericke*, consul of Magdeburg, had lately
“ practised in Germany, a way of emptying glass ves-
“ sels, by sucking out the air at the mouth of the
“ vessel plunged under water. And you may also
“ perhaps remember, that I expressed myself much
“ delighted with this experiment, since thereby the
“ great force of the external air, either rushing in at
“ the opened orifice of the emptied vessel, or vio-
“ lently forcing up the water into it, was rendered
“ more obvious and conspicuous than in any experi-
“ ment that I had formerly seen. And though it may
“ appear from some of those writings I sometimes
“ shewed your lordship, that I had been solicitous
“ to try things upon the same grounds, yet in re-
“ gard this gentleman was beforehand with me in
“ producing such considerable effects, by means of
“ the extraction of air, I think myself obliged to
“ acknowledge the assistance and encouragement,
“ which the report of his performance hath afford-
“ me. But as few inventions happen to be at first
“ so compleat, as not to be either blemished with
“ some deficiencies needful to be remedied, or other-
“ wise capable of improvement, so when the engine
“ we have been speaking of, comes to be more at-
“ tentively considered, there will appear two very
“ considerable things to be desired in it. For first,
“ the wind-pump, as somebody not improperly calls
“ it, is so contrived, that to evacuate the vessel there
“ is required the continual labour of two strong men
“ for divers hours. And next, which is an imperfe-
“ ction of much greater moment, the receiver, or
“ glass to be emptied, consisting of one entire and
“ uninterrupted globe and neck of glass, the whole
K 3 engine

“ engine is so made, that things cannot be conveyed
“ into it, whereon to try experiments: so that there
“ seems but little, if any thing, more to be expected
“ from it, than those very few phænomena that have
“ been already observed by the author and recorded
“ by *Schottus*. Wherefore to remedy these inconve-
“ niencies, I put both Mr. *Gratorix* and Mr. *Hook*
“ to contrive some air-pump that might not, like
“ the other, need to be kept under water, and might
“ more easily be managed. And after an unsuccess-
“ ful tryal or two of ways proposed by others, Mr.
“ *Hook* fitted me with a pump anon to be describ-
“ ed.”

This air-pump of Dr. *Hook*'s contrivance was, it seems, the first that Mr. *Boyle* made use of. It was indeed more perfect than that described by *Schottus* in his *Mechanica Hydraulico-Pneumatica*, yet still it laboured with several imperfections, and was not so commodious in many respects as might be desired; particularly it was furnished but with one single receiver, always fixed to the body of the engine; which therefore it was requisite should be very capacious to be fitted for all manner of tryals. Now this great capacity of the receiver made it necessary to employ a considerable time for its exhaustion; but this was an inconvenience which could not easily be dispensed with in many experiments that required a speedy evacuation; and moreover a variety in the form of the receivers to be made use of, would better suit with the variety of the subjects which were to be enquired into. Hence I suppose it was, that after he had made his first experiments with this engine, and had published them in the form of a letter to his nephew, under the title of *Physico-Mechanical experiments touching the spring of the air and its effects*, he thought it requisite to make an alteration and improvement of his instrument before he proceeded to a further prosecution of his design.

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The description of this second air-pump of Mr. *Boyle*, may be seen in the first continuation of his *Physico-Mechanical Experiments*. It consisted, as the former, only of one single barrel, by which the receiver was evacuated; but this barrel was now contrived to be every way surrounded with water, the better to prevent any possible regrefs of the air. The receivers, which were now of several shapes and bignesses, were closed to an iron plate, upon which they were placed by the means of a soft cement, and so they could easily be removed and changed as occasion required. He had not, it seems, as yet thought of that easier expedient of fixing them to the table on which they stood, by the interposition of a wet leather.

The experiments related in the second continuation of this Honourable Author, were made with an engine different from the two former. It was the contrivance of Mr. *Papin*, whose assistance Mr. *Boyle* did also make use of in the tryals themselves. This third air-pump was much more convenient than the former, and the advantage lay chiefly in these two particulars. First, whereas the former engines had only one single barrel and one sucker or embolus, this was furnished with two barrels and two suckers, and these two suckers being alternately raised and depressed, caused the evacuation to be continual; which effect could not be obtained by a single sucker, it being necessary that the evacuation should cease during the time in which the sucker is forced in towards the bottom of the barrel. But besides this advantage of performing the operation in half the time it could be done with a single sucker, the labour also in doing it was exceedingly lessened. The chief difficulty complained of in single-barrelled pumps, is the very great resistance which the external air makes against the sucker as it is drawn outwards; and this resist-

ance increases as the receiver is more and more exhausted, the counterballance of the internal against the external air being thereby more and more diminished; so that if the barrel be of a considerable wideness, it may be impossible for the strength of any one man to work the engine any longer. Now this resistance of the external air is entirely taken off by making use of two suckers instead of one. They are so connected together by the fabrick of the instrument, that as the one descends, so the other must of necessity ascend at the same time; and consequently the resistance of the external air, hindering the ascent of the one as much as it promotes the descent of the other, by contrary effects loses its force upon both. I cannot illustrate this better than by comparing it with a ballance. If a single weight be placed at one of its extremities, we perceive a difficulty in moving the beam to make the weight ascend, and this difficulty increases as the weight is greater. But if you place another weight equal to the former at the opposite extremity, the difficulty in moving the beam will entirely cease, how great soever the two equal weights may be supposed to be.

The other particular in which this air-pump excelled the former, was the advantage of its valves. In the two first engines, whilst the sucker was drawn outwards, you was obliged at the same time to turn a stop-cock, to make way for the air in the receiver to pass from thence into the barrel; and when this air was to be excluded from the barrel, as the sucker was moved inwards, you was obliged again to turn the stop-cock, to prevent the air from reverting into the receiver, and at the same time to give it a passage outwards, a stopple or plug was to be removed, which closed the hole through which it was to pass, and then again this hole was to be stopped up and the cock to be turned again as more air was drawn
from

from the receiver ; and this labour to be repeated perpetually so long as you continued to work the pump. Now the valves which in the third air-pump supplied the place of the plug and stop-cock, were undoubtedly much more convenient, in that of themselves they opened to give the air a passage forwards, and shut to prevent its return back again. I will not detain you any longer by speaking of the various forms of the engine, and the different contrivances which have been used by others. I have not observed that any of them whose descriptions I have ever met with, has been so convenient in all respects, as the air-pump before us, which was made by that excellent operator the late Mr. *Hauksbee*. I cannot say that, in the main, it is at all different from the third of Mr. *Boyle's* ; what little alterations may be observed in it, are I think for the better. It would be a loss of time to go about to describe it by words, when we may better see the contrivance of it with our eyes. I will therefore take its several parts asunder, and then endeavour to make you understand the use of each, and the operation of the whole, as clearly and distinctly as I can.

LECTURE XIII.

An account of the several successive degrees in which the air is expanded and compressed by the air-pump and condenser.

AT our last meeting we took a particular view of the several parts of which our engines consist. I shall therefore suppose you to be sufficiently acquainted with the fabrick and contrivance of them, and to understand in general the manner of their operations. I say in general, because there are some particulars which yet remain at this time to be discussed

courfed of; which may alfo very well deferve your confideration, and will be of good ufe in order to frame juft and true apprehenfions of the experiments which will hereafter be made. I fhall begin with the air-pump, and reprefent to you by what degrees the air contained in the receiver is exhausted.

It may perhaps, upon the firft view, feem not improbable that an equal evacuation is made at each ftroke of the pump, and confequently that the receiver may after a certain number of ftrokes be perfectly exhausted; for it muft be allowed, if an equal quantity of air is taken away at every ftroke, that the receiver will in time be perfectly exhausted, how fmall foever thofe equal quantities, which are continually taken away, may be fupposed to be. Thus if the air which goes out of the receiver at each turn of the pump, be but the hundredth part of what was at firft included in the receiver, it is certain that a total evacuation will be made after an hundred turns. That things are thus, may at firft view I fay, feem not improbable, but if we confider the matter more nearly we fhall find it to be far otherwife.

What I fhall endeavour to make out to you is this; that the quantities exhausted at every ftroke are not equal, but are perpetually diminifhed, and grow leffer always fo long as you continue to work the pump; that no receiver can ever be perfectly and intirely evacuated, how long time foever you employ for that purpofe, notwithstanding that the engine be abfolutely free from all defects and in the greateft perfection which can be imagined. It may appear to be a paradox, that a certain quantity of the air in the receiver fhould be removed at every turn of the pump, and yet that the whole can never be taken away; but I hope I fhall eafily fatisfy you that it is not a miftake. Laftly, that I may not feem too much to depreciate the value of our engine, I have this further
to

to say for it, that though it be impossible by its means to procure a perfect *vacuum*, yet you may approach as near to it as you please. By a perfect *vacuum* I mean in respect of air only, not an absolute vacuity in respect of every thing which is material; for not to mention what other subtle bodies may possibly be lodged in our emptied receivers, it is matter of fact that the rays of light are not excluded from thence.

In order to make out these assertions I shall in the first place lay down this rule. That the quantity of air which is drawn from the receiver at each stroke of the pump, bears the same proportion to the quantity of air in the receiver immediately before that stroke, as the capacity of the barrel into which the air passes from the receiver, does to the capacity of the same barrel and the capacity of the receiver taken together.

You may remember that in each barrel there are two valves, whereof the lower is placed at the bottom of the barrel, and the upper is fixed upon the embolus or sucker. Now the hollow space which lies betwixt these valves, when the embolus is raised as high as it can go, is what I call the capacity of the barrel; for the other part of the cavity of the barrel, which is above the embolus and the upper valve, is of no use in evacuating the receiver, and therefore ought not here to be considered. Upon a like account, by the capacity of the receiver I mean, not only the space immediately contained under the receiver, but also all those other hollow spaces which communicate with it, as far as to the lower valves: such you may remember are the cavity of the pipe which conveys the air to the barrels, and the cavity in the upper part of the gage above the quicksilver. These additional spaces are very small and inconsiderable, yet if we would be exact, they also must be taken

taken into the account and looked upon as parts of the receiver.

Now to understand the truth of the rule, we must observe that as the embolus is moved upwards from the bottom of the barrel, it would leave a void space behind it, but this effect is prevented by the rushing in of air from the receiver. The air, you know, by its elasticity is always endeavouring to expand itself into larger dimensions, and it is by this endeavour that it opens the lower valve, and passes into the hollow part of the barrel as the embolus gives way to it, and this it will continue to do, till it comes to have the same density in the barrel as in the receiver. For should its density in the barrel be less than in the receiver, its elastick force, which is proportionable to its density, would be less also, and therefore it must still give way to the air in the receiver, till at length the densities become the same. The air then which immediately before this stroke of the pump, by which the sucker is raised, was contained in the receiver only, is now uniformly diffused into the receiver and the barrel; whence it appears that the quantity of air in the barrel, is to the quantity of air in the barrel and receiver together, as the capacity of the barrel, is to the capacity of the barrel and receiver together. But the air in the barrel is that which is excluded from the receiver by this stroke of the pump, and the air in the barrel and receiver together, is what was in the receiver immediately before the stroke; therefore the truth of the rule is very evident, that the quantity of air which is drawn from the receiver at each stroke of the pump, bears the same proportion to the quantity of air in the receiver immediately before that stroke, as the capacity of the barrel into which the air passes from the receiver, does to the capacity of the same barrel and the capacity of the receiver taken together.

To

To illustrate this further by an example, let us suppose the capacity of the receiver to be twice as great as the capacity of the barrel; then will the capacity of the barrel, be to the capacity of the barrel and receiver together, as 1 to 3; and the quantity of air exhausted at each turn of the pump, is to the quantity of air which was in the receiver immediately before that turn, in the same proportion. So that by the first stroke of the pump, a third part of the air in the receiver is taken away, by the second stroke a third part of the remaining air is taken away, by the third stroke a third part of the next remainder is exhausted, by the fourth a third part of the next, and so on continually; the quantity of air evacuated at each stroke, diminishing in the same proportion with the quantity of air remaining in the receiver immediately before that stroke: for it is very evident that the third part, or any other determinate part of any quantity must needs be diminished in the same proportion with the whole quantity itself. And this may suffice for the proof of what I asserted in the first place, that the quantities exhausted at every stroke are not equal but are perpetually diminished.

I shall now proceed to shew, that the air remaining in the receiver after every stroke is diminished in a geometrical progression. It has been proved that the air remaining in the receiver after each stroke of the pump, is to the air which was in the receiver immediately before that stroke, as the capacity of the receiver is to the capacity of the barrel and receiver taken together; or in other words, that the quantity of air in the receiver, by each stroke of the pump, is diminished in the proportion of the capacity of the receiver to the capacity of the barrel and receiver taken together. Each remainder is therefore evermore less than the preceding remainder in the same given

given ratio ; that is to say, these remainders are in a geometrical progression continually decreasing.

Let us return again to our former example, which may afford a somewhat different light into this matter. The quantity exhausted at the first turn, you remember, was a third part of the air in the receiver, and therefore the remainder will be two thirds of the same ; and for the like reason the remainder after the second turn will be two thirds of the foregoing remainder, and so on continually ; the decrease being always made in the same proportion of 2 to 3 ; consequently the decreasing quantities themselves are in a geometrical progression.

It was before proved that the quantities exhausted at every turn did decrease in the same proportion with these remainders ; therefore the quantities exhausted at every turn are also in a geometrical progression. Let it then be remembered, that the evacuations and the remainders do both decrease in the same geometrical progression.

If the remainders decrease in a geometrical progression, it is certain you may, by continuing the agitations of the pump, render them as small as you please, that is to say, you may approach as near as you please to a perfect *vacuum*. But notwithstanding this, you can never entirely take away the remainder. If it be said that you may, I prove the contrary thus. Before the last turn of the pump, which is said wholly to take away the remainder, it must be confessed there was a remainder ; this remainder, by that last turn of the pump, will only be diminished in a certain proportion, as has been before proved ; therefore it was falsely said to be totally taken away.

It may not be improper in this place to say something concerning the gradual ascent of the quicksilver in the gage, upon which we have made some experiments. You have observed that as we continue

to pump, the quicksilver continues to ascend, approaching always more and more to the standard altitude in the weather-glass, which you know is about $29\frac{1}{2}$ inches, being a little under or over according to the variety of seasons. What I shall now endeavour to make out to you is this; that the defect of the height of the quicksilver in the gage from the standard altitude, is always proportionable to the quantity of air, which remains in the receiver; that the altitude itself of the quicksilver in the gage, is proportionable to the quantity of air which has been exhausted from the receiver; that the ascent of the quicksilver upon every turn of the pump, is proportionable to the quantity evacuated by each turn.

In order to understand these assertions you are to consider, that the whole pressure of the atmosphere upon the cistern of the gage, is equivalent to, and may be ballanced by, a column of quicksilver of the standard altitude. Therefore when the quicksilver in the gage has not yet arrived to the standard altitude, it is certain the defect of quicksilver is supplied by some other equal force, and that force is the elastick power of the air yet remaining in the receiver; which communicating, as you remember, with the upper part of the gage, hinders the quicksilver from ascending, as it would otherwise do, to the standard altitude. The elasticity of the air in the receiver is then equivalent to the weight of the deficient quicksilver; but the weight of that deficient quicksilver is proportionable to the space it should possess, or to the defect of the height of the quicksilver in the gage from the standard height; therefore the elasticity of the remaining air is also proportionable to the same defect. And since it was formerly proved, that the density of any portion of air is always proportionable to its elasticity, and the quantity in this case is proportionable to the density, it follows that the
quantity

quantity of air remaining in the receiver, is proportionable to the defect of the quicksilver in the gage from its standard altitude, which was the first thing to be proved.

Hence it follows that the quantity of air which was at first in the receiver before you began to pump, is proportionable to the whole standard altitude; and consequently the difference of this air, which was at first in the receiver and that which remains after any certain number of turns, that is, the quantity of air exhausted, is proportionable to the difference of the standard altitude and the beforementioned defect, that is, to the altitude of the quicksilver in the gage after that number of turns; which was the second thing to be proved.

And from hence it follows that the quantity of air exhausted at every turn of the pump, is proportionable to the ascent of the quicksilver upon each turn, which was the last thing to be made out. And these conclusions do very well agree with the experiments, which shewed us the quantity of air that was exhausted, by the quantity of water which afterwards supplied the vacant place of that air in our receiver (*a*).

Let it then be remembered, that the quantity exhausted at each turn, is proportionable to the ascent of the quicksilver upon that turn; that the whole quantity exhausted from the time you began to pump, is proportionable to the whole altitude of the quicksilver; that the quantity remaining in the receiver is proportionable to the defect of that altitude from the standard.

To come now to the application of the other experiments which we made this day. We found, by measuring, that the several ascents of the quicksilver

(*a*) This receiver, like the bottle described in Exp. 1. pag. 8, had a stop-cock cemented to it, to hinder the return of the exhausted air, and to admit water in its stead.

in the gage, upon every turn of the pump, were diminished in a geometrical progression, and it has just now been proved that the quantities of air exhausted at each turn are proportionable to those ascents. Therefore we may safely conclude from experiment also, what we before collected by a train of reasoning, that the quantities of air exhausted at every turn of the pump, are diminished continually in a geometrical progression.

Furthermore, since those ascents are the differences of the defects from the standard altitude, upon every successive turn of the pump, it follows that the defects also are in the same decreasing geometrical progression. For it is a general theorem, that all quantities whose differences are in a geometrical progression, so long as the quantities continue to have any magnitude, are themselves also in the same geometrical progression. The defects being then in a decreasing geometrical progression, and the quantities of air remaining in the receiver being proportionable, as was lately proved, to the defects, it follows from the same experiments, that the quantities of air which remain in the receiver after every turn of the pump, do decrease in a geometrical progression; which was the other thing concluded also by a train of reasoning.

Before I dismiss the consideration of the air-pump, it remains that I add something concerning the use of the two tables, which I have put into your hands. They are designed to shew the number of turns of the pump, which are requisite to rarefie, in any given proportion, the air contained under any receiver. The first table in particular is fitted for receivers whose capacity is the same with the capacity of the barrel, and the numbers of the first column of it express the degrees of rarefaction, as those over against them in the second column express the number of turns, with

their decimal parts, which are requisite to produce those degrees of rarefaction. Thus for example, if it were required to rarefie the air, under such a receiver, an hundred times above its natural rarity, I seek for the number 100 in the first column, and over against it in the second I find the number 6.644, by which I understand that the air will be rarefied an hundred times by 6 turns of the pump and 644 thousand parts of a turn. So if it were desired to rarefie the air under the same or an equal receiver 10 thousand times more than in its natural state, I perceive there will be 13 turns and 288 thousand parts of a turn requisite for that purpose.

TABLE I.					
Rarity	Number of turns	Rarity	Number of turns	Rarity	Number of turns
1	0	60	5.907	900	9.814
2	1	64	6	1000	9.966
3	1.585	70	6.129	1024	10.
4	2	80	6.322	2000	10.966
5	2.322	90	6.492	2048	11.
6	2.585	100	6.644	3000	11.551
7	2.807	128	7	4000	11.966
8	3	200	7.644	4096	12.
9	3.170	256	8	5000	12.288
10	3.322	300	8.229	6000	12.551
16	4	400	8.644	7000	12.773
20	4.322	500	8.966	8000	12.966
30	4.907	512	9	8192	13.
32	5	600	9.229	9000	13.136
40	5.322	700	9.451	10000	13.288
50	5.644	800	9.644	16384	14.

The receivers which we shall have occasion to make use of in our experiments, are generally much bigger

bigger than the capacity of each barrel of the pump, and by being bigger, will require a greater number of turns than those set down in the second column, to rarefie the air in the degrees which are expressed in the first column. It may perhaps at the first view, seem not unreasonable to think that the number of turns requisite to rarefie the air in any certain degree, should exceed the numbers of the second column, in the same proportion by which the capacity of the receiver exceeds the capacity of the barrel. But if the matter be examined more closely, it will be found, that the number of turns do not increase in so great a proportion as the capacity of the receiver does.

What that proportion is, by which the number of turns is truly increased, as the capacity of the receiver becomes bigger, may be seen by the second table, whose first column expresses the proportion of the receiver to the barrel, as the second does the proportion of the true number of turns to those set down in the first table. The use of it will be more clearly understood by an example or two.

Let us suppose the capacity of the receiver to be 10 times greater than the capacity of the barrel, and that we would find how many turns are requisite to rarefie the air under such a receiver 100 times more than it is naturally rarefied.

By the first table we find, as was said above, that if the receiver were equal to the barrel, the number of turns would be 6.644. But the receiver is 10 times greater. Find therefore the number 10 in the first column of the second table, and over against it you will see the number 7.273 in the second column of the same table; by which you perceive that as the receiver is increased in a decuple proportion, the number of turns are increased not so much, but only somewhat more than in a septuple proportion. There-

fore the true number of turns will be found by multiplying the number 6.644 by the number 7.273, and will consequently be 48.322.

TABLE II.			
Capacity of the receiver	Multiplier	Capacity of the receiver	Multiplier
1	1.	60	41.934
2	1.710	70	48.866
3	2.409	80	55.798
4	3.106	90	62.729
5	3.802	100	69.661
6	4.497	200	138.976
7	5.191	300	208.291
8	5.885	400	277.605
9	6.579	500	346.920
10	7.273	600	416.235
20	14.207	700	485.549
30	21.139	800	554.864
40	28.071	900	624.179
50	35.003	1000	693.494

So if it were desired to find the number of turns of the pump, which must be made to rarefie the air 10 thousand times above its natural state, in a receiver which is 50 times bigger than the capacity of the barrel; over against 10000 in the first table I find 13.288 and over against 50 in the second table I find 35.003, which multiplied together make 465.12; this therefore is the number of turns requisite for the purpose. You need not be solicitous about the fractions which are above any certain whole number of turns; they do not mean, that the handle of the pump is to be moved justly such a part of a turn as they seem

seem to denote, for strictly speaking it need not be moved altogether so much, but the difference is inconsiderable, and it would be a loss of time to insist more particularly about it. It was necessary to set down the fractions in the tables that no whole number of turns might be lost in the product, when you come to multiply them together; but when you have found the product, the fractions belonging to it need not be considered.

In making these tables, that they might not be too large, you see I have omitted several intermediate numbers. However they are sufficient for the purpose for which I designed them; which was to give you clearer notions of the operation of our engine. I should here explain to you the grounds upon which they were computed, but I fear the difficulty of the subject would not permit me to be generally understood. I shall therefore omit the doing of it, (*b*) and

(*b*). In a receiver whose capacity is to that of the barrel of the pump, as c to 1, the air will be rarefied r times by a number of turns equal to $\frac{\log. r.}{\log. \frac{c+1}{c} - \log. c.}$

For by any one of the turns, the air will be rarefied or dilated in the ratio of the space c to the space $c+1$, or of 1 to $\frac{c+1}{c}$; and therefore will have as many such equal and successive rarefactions, as there are equal and successive ratio's in this series $1, \left(\frac{c+1}{c}\right)^1, \left(\frac{c+1}{c}\right)^2, \left(\frac{c+1}{c}\right)^3, \dots$ continued to the term $\left(\frac{c+1}{c}\right)^n$, whose index n is the whole number of turns. Consequently the total ratio of all the rarefactions, for which we put 1 to r , is the same as the ratio of 1 to $\left(\frac{c+1}{c}\right)^n$, that is, of the first term of the series to the last. Therefore $r = \left(\frac{c+1}{c}\right)^n$, and by the noted properties of logarithms, the $\log. r = n \times \log. \frac{c+1}{c} = n \times \log. \frac{c+1}{c} - \log. c.$ whence $\frac{\log. r}{\log. \frac{c+1}{c} - \log. c} = n.$

Corol.

only observe to you of the first table, that if you take any numbers in the first column which are in a geometrical progression, the correspondent numbers of the second column will be in arithmetical progression. It may also be observed of the second table that the disproportion of the correspondent numbers does continually increase from the beginning to the end, how far soever it be continued, but yet does never exceed the disproportion of 13 to 9.

It is time now that we proceed to the condenser. This instrument will not require much to be said concerning it. When I assert that equal quantities of air, namely, as much as the barrel can naturally contain, are intruded into the receiver at each stroke of the forcer, the thing is so very obvious that I believe I need not go about to prove it. For you cannot but easily understand, that as the embolus or forcer is drawn upwards from the bottom of the barrel, there is a vacuity left behind it, till such time as it comes to get above the little hole, which is made in the side of the barrel towards the top of it. For then the external air is permitted to pass freely through that hole into the aforefaid void space, and consequently the barrel will then have as much air in it, as it can

Corol. 1. Hence in Table I, where $c=1$, the air will be rarefied r times by a number of turns equal to $\frac{\log. r}{\log. 2}$.

Corol. 2. Call that number t , and in Tab. II, where c has any proposed value, put m for the corresponding multiplier; then since the author makes $m t = n$, we have $m = \frac{n}{t} = \frac{\log. 2}{\log. c+1-\log. c}$.

In the author's example $r=100$ & $c=10$, whence by *Corol. 1*, $t = \frac{\log. 100}{\log. 2} = \frac{2.00000}{0.30103} = 6.644$ turns; and by *Corol. 2*, $m = \frac{\log. 2}{\log. 11-\log. 10} = \frac{0.3010300}{0.0413927} = 7.273$, & $m t = n = 48.322$ turns.

natu-

naturally contain. And as the forcer is moved downwards, this air is compressed, and by compression is more and more condensed, till at length the force of its elasticity becomes greater than the elastick force of that which is contained within the receiver, and thereby it will open the valve and make way for itself to enter totally into the receiver, as it is continually pushed forwards by the descending embolus. Since then the quantities intruded at each stroke of the forcer are equal, it manifestly appears that the quantities in the receiver, and consequently the degrees of condensation, do increase in an arithmetical progression.

Let us now examine by what steps the quicksilver in the gage advances at each stroke. What I shall endeavour to prove as to this matter is this, that as the quicksilver is moved forwards in the gage upon every successive stroke of the forcer, the spaces at the end of the gage, which are yet left free from the quicksilver, do decrease in a musical progression.

But in the first place it may not be amiss to explain in some measure the nature of musical progressions, since these are not generally so well understood as those which we call arithmetical and geometrical progressions. In order to do this, I shall propose an instance which first gave occasion for the name.

It is a thing well known among musicians, if three chords or strings, in all other respects alike, be of different lengths, and those lengths be to each other in proportion as the numbers of 6, 4 and 3, that the sounds of those strings will express the principal and most perfect of the musical concords, namely, an eighth, a fifth and a fourth. Thus the sound of the last will be an octave to the sound of the first, and the sound of the second a fifth to the sound of the first, and the sound of the last a fourth to the sound of the second. Hence these numbers 6, 4 and 3, which

express the proportions of those musical strings, were said not improperly to be in a musical progression. Now it was easy to be observed, that these numbers were reciprocally proportional to three other numbers respectively, viz. 2, 3 and 4, which were in an arithmetical progression; and thence it came to pass, that any other series of numbers was said to be in a musical progression, which had the same property of being reciprocally proportional to a series of numbers in arithmetical progression.

That therefore is a series of musical proportionals which is reciprocal to another series of arithmetical proportionals.

But besides this, you may observe another property belonging to the above mentioned numbers 6, 4 and 3, viz. that the first is to the third, as the difference of the first and second, is to the difference of the second and third. And this property does equally belong to all other numbers, which are reciprocally as a series in arithmetical progression, that is, to all other numbers which are in a musical progression.

Hence if any two succeeding terms be given, the third may be found by dividing the product of the first and second, by the difference which arises in subtracting the second from the double of the first. Thus in the progression 6, 4 and 3, the product of the first and second terms 6 and 4 is 24, and the difference which arises by subtracting the second term 4 from 12, the double of the first, is 8, and the quotient which emerges by dividing the product 24 by the difference 8 is 3, the third term in the progression required.

I shall now go on to shew that the spaces unpossessed by the quicksilver at the end of the gage, decrease in a musical progression.

It must be observed therefore, that the quicksilver of the gage is contiguous on one side to the air
within

within the receiver, and on the other side to the air which is shut up at the end of the gage, and that the density of the air in both places is equal. For were the density of the air in the receiver greater than the density of the air at the end of the gage, its elastick force would also be greater, and by that excess of force the quicksilver would be moved on further towards the end of the gage, till the forces, and consequently the densities, became equal. After the same manner if the density of the air at the end of the gage, were greater than the density of the air within the receiver, the quicksilver would be moved backwards from the end of the gage, till the densities became equal. It is manifest therefore that the densities are equal in both parts when the quicksilver in the gage is at rest. Therefore since the density of the air in the receiver, upon every successive stroke of the forcer, was increased in arithmetical progression, it follows that the density of the air at the end of the gage, is likewise increased in the same arithmetical progression. But the space which that air possesses, is diminished in the same proportion by which the density is increased, or in other words, the spaces are reciprocally as the densities, therefore the spaces are reciprocally as a series of terms in arithmetical progression; which is the same thing as to say, the spaces are in a musical progression. And this conclusion we found also to agree with our experiments.

LECTURE XIV.

A parcel of air weighed in a ballance; its specific gravity to that of water determined thereby.

THE gravitation of the air has in the foregoing week been sufficiently proved by several different methods. We have seen a great variety of appearances

pearances which are very clearly and naturally accounted for upon that principle, and cannot be explained upon any other. We might therefore very justly conclude it to be altogether true and exactly agreeable to nature, upon the force of that evidence alone. But the immediate evidence of sense has always something in it, which does more powerfully affect and convince us, than the united strength of the greatest number of inferences drawn out by a series of reasoning. And upon this account we may say, that the experiment we have now been making, has afforded us a clearer and more cogent proof of the weight of the air, than any of those considerations we have hitherto been engaged in, or even than all of them taken together. For what can be further expected or desired to evince an heavy body to be really such, than to feel (if I may say so) the weight of it in the ballance. This experiment itself or some other to the same effect, might at any time so easily have been tried, that I believe you will be much more inclined to wonder, how it could have been possible for any sect of philosophers to have doubted of the gravitation of the air, than to doubt of it in the least yourselves.

It must be confessed that *Aristotle* himself does somewhere in his writings assert this gravitation, and to prove his assertion he appeals to the experiment of a bladder full blown; which, says he, weighing more than the same bladder when it was flaccid, is a manifest token of the weight of the air contained in it. But it is certain, however unreasonable it may seem, that his followers departed from their master, and maintained the contrary for several ages together. *Galileo* seems to have been the first among the modern philosophers, who durst venture to oppose them in this matter. I formerly gave you an account of the observation he had made concerning pumps;
this

this was a sufficient hint to a person of his sagacity, to call into question the commonly received doctrine of the schools. But the inquisitive genius of this great man was not long to be held in suspense. He soon resolved upon proper experiments which might afford him a full and perfect satisfaction in this business. What those experiments were, I am now to tell you.

He took a glass vessel of a large capacity, having a very narrow neck, to which he applied a cover of leather and fastened it as close as was possible to the neck. Through the middle of this cover he put a slender tube, closing the leather to it as exactly as he could. Then with a syringe he forced into the glass vessel, through the tube, as great a quantity of air as he conveniently might, so as not to endanger the breaking of the glass. This being done he weighed the vessel, with all this compressed air in it, as carefully as he could, by the help of a most exact ballance; making use of very fine sand for a counterpoise. Afterwards, by uncovering the tube, he permitted the air, which had been with violence forced into the vessel, to make its escape out again; and then he applied the vessel to the ballance a second time, and finding it to be lighter than before, he took away part of the sand, till at length he had reduced the beam to an equilibrium again. By this means he was entirely convinced of the ponderosity of the air in general, and was satisfied that the weight of so much of it as had escaped upon opening the vessel, was equal to the weight of that sand which he had taken away and reserved apart by itself. This indeed was very evident, but yet it was not possible from this experiment, to form any conclusion concerning the precise and determinate weight of any particular quantity of air, in comparison with the weight of other bodies: for it could not certainly be known,

known, what the quantity of that air was which had made its escape, and which was equal in weight to the sand he had reserved. In order therefore to pursue this matter somewhat further, he contrived two other different methods of making the experiment. The first was after this manner.

He took another vessel in all respects alike to the former, which had also a cover of leather fixed to it. Through this cover he caused to pass into this second vessel, the end of the tube which stood out of the former vessel and was closed up, and at the same time he took care to bind the cover of the second vessel as exactly as was possible to the tube. This being done, the necks of the vessels respected each other, and had a communication by means of the tube, so soon as the closed end of it, which lay in the second vessel, was opened. Now the end of the tube was opened by a long and slender iron-pin, passing through a small hole made at the bottom or opposite end of the second vessel. But I ought to tell you, that before these vessels were thus joined together, the second was filled with water, and the weight of the first, with all its included and compressed air, was found by a counterpoise of sand as before. Things then being thus disposed and prepared, and the end of the tube being opened by means of the iron-pin, it is easy to understand that the compressed air will rush forth with violence into the second vessel, till that which remains behind in the first vessel be reduced to its natural density. Now as the air forces its way into the vessel filled with water, it must necessarily happen, that part of this water will be expelled, through the hole at the bottom of the vessel, to make way for the air; and the bulk of the water which is expelled, will be equal to the bulk of the air which came forth from the first vessel into the second. The weight of that air, which came out from the first vessel,

vessel, is to be found by weighing the first vessel again after that air is gone out of it; and this weight of air, compared with the weight of that equal bulk of water which was expelled and reserved, will give the proportion of the specifick gravity of air to the specifick gravity of water. By this method *Galilæo* tells us, he found that air, was about 400 times lighter than water.

The other method which he proposes, is somewhat more expeditious, it requiring only one vessel, which must be fitted up as the former of those two, which have been just now described. But here instead of forcing in more air than the vessel does naturally contain, he chuses rather to force in water, whereby the air contained in the vessel is compressed and condensed, care being taken that no part of it escape out, as the water is forced in. Supposing then a sufficient quantity of water to be forced into the vessel, which may conveniently enough be as much as will fill $\frac{3}{4}$ of it, the whole must be weighed with great exactness. Then the neck of the vessel is to be placed upwards, and the tube being opened, the air will be at liberty to expand itself, and such a part of it will go out, whose bulk is equal to the bulk of water which was forced in. This being done, the vessel must be weighed again, and this last weight will be so much less than the former, as the weight of the air which went out amounts to; and consequently the specifick gravity of air will be to the specifick gravity of water, as that defect is to the weight of the water which was forced into the vessel.

Such were the contrivances of this admirable Italian philosopher at a time when the rest of the world had altogether different notions of these matters, and long before our air-pumps or barometers were invented. It is a satisfaction to understand by what methods these enquiries first began. But besides this, you have also another advantage from the account I have
been

been giving you ; my meaning is, that you will from hence be enabled to frame to yourselves apprehensions of various other ways of doing the same thing, different from those I have been relating, or that which we have been practising. It would be endless to describe the several methods which have been proposed by curious and inquisitive men, or to tell you what methods I could myself contrive to the same purpose. The first experiment of this nature which was generally taken notice of, and became in its time very famous, was that of *Mersennus*. Omitting therefore all others, I shall content myself with the account of his manner of trial.

He procured to himself an *Æolopile*, being an hollow globe of brass with a very slender neck. This he placed in the fire till it became red-hot, and immediately weighed it by a ballance whilst it remained so. Afterwards he let it cool and then weighed it again ; and finding its weight to be greater than before, he concluded that the excess was the weight of that air which had been expelled by the heat, and had been permitted to return again upon the cooling of the globe. Thus he was satisfied that the air was a ponderous body, but in what measure it was so, he could not by this experiment alone determine. He therefore repeated the trial again, and found the weight of the globe when it was red-hot to be the same as before. Then he placed the neck of it under water and suffered it to cool in that posture. Which being done he found his globe to be almost filled with water, and knowing the bulk of that water to be the same with the bulk of the air which was expelled with the heat, by weighing that water and comparing its weight with the weight of the air found by the former experiment, he concluded the specific gravity of air to be about 1300 times less than the specific gravity of water.

You

You cannot but take notice of a very great difference between this conclusion and that of *Galilæo*, which I mentioned above. *Galilæo* makes the air to be 400 times lighter than water, and *Mersennus* makes it to be 1300 times lighter. If we take a mean between these conclusions we must say it is 850 times lighter; and this agrees very well with later and more exact observations. You have already seen what the proportion was which we deduced from our experiment. But I ought to tell you that if our bottle had been bigger, we might the more securely have depended upon it.

We have formerly in some of our first courses, made use of a bottle about 8 times as capacious as that with which we made our present trial, and I can assure you we always found the proportion of water to air to be between the proportions of 800 and 900 to 1, but it generally approached somewhat nearer to 900. That larger bottle was procured for us by the late Mr. *Hauksbee*, who himself made the experiment before the Royal Society with the same, at a time when I happened to be there present. I can therefore witness for the diligence and care with which he made it. The proportion which was found by his experiment was that of 885 to 1. The method by which he proceeded was the very same with ours, and therefore I may suppose you to understand it perfectly. But because the recital of it may serve to fix it the better in your memories, I shall here subjoin his own account of the whole process.

“ I took, says he, a bottle which held more than
“ 3 gallons, but how much more, we have no occa-
“ sion at present to take notice of, and of a form
“ something oval; which figure I made choice of
“ for the advantage of its more easy libration in wa-
“ ter. Into this bottle I put as much lead as would
“ serve to sink it below the surface of the water. And
“ the

“ the reason why I chose rather to have the weight
“ of lead enclosed within the bottle, than fixed any
“ where on the outside, was, to prevent the incon-
“ veniencies which in the latter case must needs have
“ arose from bubbles of air : for these bubbles would
“ have inevitably adhered to, and lurked in great
“ plenty about the body of the weight, had it been
“ placed on the outside, which must have caused
“ some errors in the computations of an experiment
“ that required so much exactness and nicety. These
“ things thus provided, the bottle containing com-
“ mon air closed up, was by a wire suspended in
“ the water, at one end of a very good ballance, and
“ was counterpoised in the water by a weight of
“ $358\frac{1}{2}$ grains in the opposite scale. Then being
“ taken out of the water and screwed to the pump,
“ in 5 minutes time it was pretty well exhausted, the
“ mercury in the gage standing at near $29\frac{1}{2}$ inches.
“ After which, having turned a cock that screwed
“ both to the bottle and the pump, and so prevent-
“ ed the air's return into it again, it was taken off
“ from the pump, and suspended as before, at one
“ end of the ballance in the water. And now the
“ weight of it was but $175\frac{1}{2}$ grains ; which therefore
“ subtracted from $358\frac{1}{2}$ grains (the weight of the
“ bottle with the enclosed air, before it had been
“ applied to the air-pump) gave for the difference
“ 183 grains ; which difference must consequently
“ be the weight of the quantity of air, drawn from
“ the bottle by the pump. Having thus determined
“ the weight of the exhausted air, the cock was open-
“ ed under water, upon which the water was at first
“ impelled with a considerable violence into the bot-
“ tle (though this force abated gradually afterwards)
“ and continued to rush in, till such a quantity was
“ entered, as was equal to the bulk of the air with-
“ drawn. And then the bottle, being examined by
“ the

“ the ballance again, was found to weigh 162132
 “ grains ; from which substracting $175\frac{1}{2}$ grains (the
 “ weight of the bottle with the small remainder of
 “ included air, after it was taken from the air-pump)
 “ there remains $161956\frac{1}{2}$ grains, for the weight of
 “ a mass of water equal in bulk to the quantity of air
 “ exhausted. So that the proportion of the weights
 “ of two equal bulks of air and water, is as 183 to
 “ $161956\frac{1}{2}$, or as 1 to 885.

“ There are two things particularly observable
 “ in this experiment. First, that in making it after
 “ this manner, one need not be very solicitous a-
 “ bout a nice and accurate exhaustion of the receiv-
 “ er: the success of the experiment does not at all
 “ depend upon it ; for to what degree soever the ex-
 “ haustion be made, it must still answer in propor-
 “ tion to the quantity taken out. Neither can any
 “ more water possibly enter into the receiver, than
 “ what will just supply the place, and fill up the
 “ room, deserted by the exhausted air. Secondly,
 “ the season of the year is to be considered in making
 “ this experiment. I made it in the warm month of
 “ May, the mercury in the barometer standing at
 “ the same time at $29\frac{7}{8}$ inches. From whence it is
 “ reasonable to conclude, that a sensible difference
 “ would arise, were it to be tried in the months of
 “ December or January, when the state and consti-
 “ tution of the air is usually different from what it
 “ is in the forementioned month”.

Thus far Mr. *Hawksbee* ; and here I might con-
 clude what seems to me sufficient to have been said
 upon this occasion. But before I do so, it may not be
 amiss in this place to make our enquiries once more
 concerning the state of the atmosphere and the dif-
 ferent degrees by which the air is rarefied at different
 altitudes above the surface of the earth. You remem-
 ber it was proved in the foregoing week, that the

density of the air was diminished in a geometrical progression as the altitude of it was increased in an arithmetical progression. The truth of that rule depends upon this supposition, that the gravity of bodies is the same at all distances from the center of the earth. But it has been proved and put beyond dispute by Sir *Isaac Newton*, in his *Principia*, that the gravity of bodies is not exactly the same at all distances from the center, but is diminished as the distance increases; so that the quantity of it is always reciprocally proportionable to the square of the distance. From hence it easily appears, that when the altitude of the air above the surface of the earth, is very great and very considerable in respect of the earth's semidiameter, the rule which I formerly gave you will be far from being true; but if the altitude be small and inconsiderable, as the altitudes of our highest mountains must be confessed to be, it will still be sufficiently exact, and as such it is proposed by Dr. *Halley* in the *Philosophical Transactions*, and by Dr. *Gregory* in his *Astronomy*, and generally received by others without any exceptions. However, it may be worth our while to see what consequences will arise upon the truer hypothesis, which supposes, as I said above, the gravity of bodies to be diminished in the same proportion by which the square of their distance from the center of the earth is increased. In treating of this matter, I fear I shall not be generally understood, yet I hope I shall make the thing as easy as the nature of it will permit.

In FIG. 40, let *C* represent the center of the earth, *CA* its semidiameter, *AB* a part of its surface, and let the line *CAD* be produced up to the extremity of the atmosphere. In this line imagine the points *D*, *E*, *F* to be placed infinitely near to each other, and take as many other points *d*, *e*, *f* in such a manner, that the distances *dC*, *eC*, *fC* shall be reciprocally propor-

proportionable to the distances DC , EC , FC respectively, or in such manner, that the distances dC , eC , fC shall be less than the semidiameter AC , in the same proportion by which the respective distances DC , EC , FC are greater than the same semidiameter; the distances of the lesser letters from the center being diminished in the same proportion by which the distances of the corresponding greater letters from the center are increased.

Upon the points A , d , e , f erect the perpendiculars AB , dp , eq , fr , and suppose the length of these perpendiculars to be proportionable to the density of the air in A , D , E , F respectively, so that the density of the air at A shall be represented by the perpendicular AB , the density of the air at D by the perpendicular dp , the density at E by the perpendicular eq , and the density at F by fr .

This being done, I am now to prove, that if the distances CF , CE , CD be taken in a musical progression, and consequently the distances Cf , ce , cd , be in an arithmetical progression, as being reciprocally proportionable to the former distances, the perpendiculars fr , eq , dp , and consequently the densities of the air in the places F , E , D , which are analogous to the perpendiculars, will be in a geometrical progression.

In the first place then, because the distances of the lesser letters from the center, are reciprocally as the distances of their correspondent greater letters from the same, it is manifest that Cd is to Ce as CE is to CD , and consequently the difference of Cd and Ce , is to the difference of CE and CD , as Ce to CD , or (because the points E and D are supposed to be infinitely near to each other) as Ce to CE , or (because Ce is less than CA in the same proportion by which CE is greater than CA , and consequently Ce , CA and CE are continual proportionals) as CAq is to CEq .

It is evident then, that de (the difference of Cd and Ce) is to DE (the difference of CE and CD) as CAq is to CEq .

Therefore if the distance CE remain unaltered, and consequently the proportion of CAq to CEq remain unaltered, the proportion of de to DE will also remain unaltered, and consequently de will be as DE ; that is, de will be increased and diminished in the same proportion with DE .

But if DE remain unaltered, because it is always greater than de in the proportion by which CEq is greater than CAq , it follows that de must necessarily be diminished in the same proportion by which CEq is increased, and increased in the same proportion by which CEq is diminished; or in other words, it must always of necessity be reciprocally as CEq .

Whence it follows, that if neither DE nor CE remain unaltered, de will be as DE directly and as CEq reciprocally.

But the bulk of air between the places D and E is as DE , and the gravitation of the same is reciprocally as the square of CE , its distance from the center; therefore de is as the bulk and gravitation together of the same; and consequently since eq is as its density, the product of de and eq or the area $deqp$ will be as the product of its density, bulk and gravitation, that is, as its force to compress the inferior air.

And the sum of all such areas below dp will be as the sum of such forces of all the air above D , that is, as dp the density of the air at D ; for you know the density of the air is always as the force which compresses it.

Since the perpendicular dp is as the sum of all the little areas below itself, and the perpendicular eq , for the same reason, is as the sum of all below itself, it follows that the difference of eq and dp is as the difference of those sums, which difference is the area $capd$.

Thus

Thus far then we have proceeded: we have found that the difference of the perpendiculars eq and dp is as the area $eqpd$ comprehended by those perpendiculars.

Let us now suppose the distances CF , CE , CD , and so on, to be taken in a musical progression, and then, as was said above, the distances Cf , Ce , Cd , and so on, will be in an arithmetical progression; and therefore all the intervals de , ef will be equal, and consequently the areas $eqpd$, which have those equal intervals for their bases, will be as their altitudes eq .

Hence the difference of eq and dp , which was as the area $eqpd$, will be as eq , and consequently dp will be as eq . In other words, the two perpendiculars, which terminate the little area included between them, do every where bear the same given proportion to each other: that is, the proportion of fr to eq is the same with the proportion of eq to dp , and consequently the perpendiculars fr , eq , dp , and so on, are in a geometrical progression.

But these perpendiculars express the density of the air at the places F , E , D , and so onwards. Therefore those densities are also in a geometrical progression, which was the thing to be proved.

To proceed further; since Cd is to CA as CA is to CD , it follows that Ad is to AD as CA to CD , or in other words, that Ad is less than AD in the same proportion by which the semidiameter of the earth is less than the distance of the point D from the center.

Consequently to find the length of Ad , we must diminish the altitude AD in the proportion of the semidiameter of the earth to the sum of the semidiameter and the altitude, for which reason I shall call Ad the diminished altitude of the point D ; and upon the same account Ae may be called the diminished altitude of the point E , and Af the diminished

altitude of the point F ; and so if b be the point which corresponds as above to the point H , Ab will be the diminished altitude of the point H .

Now it is easy to observe that as the distances Cd , Ce , Cf are in arithmetical progression, so are also the diminished altitudes Ad , Ae , Af .

And from hence there arises this Theorem. That if the diminished altitudes be taken in arithmetical progression, the densities of the air will be in a geometrical progression.

Therefore if the rarity of the air at any one altitude, suppose at H , be known, you may easily enough find its rarity at any other altitude, suppose at D . For as the diminished altitude of the point H , is to the diminished altitude of the point D , so will the logarithm of the air's rarity at H , which is supposed to be known, be to the logarithm of the air's rarity at D , which was to be found.

The whole difficulty of the business is therefore reduced to this; to find the rarity of the air at some one altitude as at H . This may be done as I formerly shewed you, by carrying the barometer to the top of some very high mountain, and observing the descent of the quicksilver. Such were the experiments made upon the Puy de Domme in France and Snowdon Hill in Wales, which I made use of the last week when I discoursed of this subject.

But the method I shall now describe to you is more expeditious, and depends upon the experiment which we made this day. It appears, as I said, by many such experiments compared together, that the weight of air is to the weight of water as 1 to about 850. Therefore a column of air whose height is 850 inches or 70 feet and 10 inches, will be equal in weight to a column of water upon the same basis, whose height is 1 inch. Let us suppose that AH , the height of the point H above the surface of the earth is 70 feet and

10 inches; then because the standard height of water in the Pascalian tube is 34 feet or 408 inches, and this height of water is a ballance to the pressure of the whole atmosphere upon the surface of the earth, it is manifest that the weight of the whole column of air, which is superior to the point A , is equal to the weight of a column of water upon the same basis, whose height is 408 inches. Take from the weight of the whole column of air, the weight of that part of the column which reaches from A up to H , and which was shewn to be equal to one inch of water, and the weight of the remaining part of the column, which is above the point H , will be equal to the weight of 407 inches of water. Therefore the force with which the air at A is compressed, is to the force with which the air at H is compressed, as 408 to 407; and the rarity of the air at H , is to the rarity of the air at A , in the same proportion.

You may perceive that this method supposes the air to be of the same density in every part of the space AH , which is not exactly true; but in so small an altitude as that of 70 feet, the error is altogether insensible. However, if you have a mind to proceed with the utmost accuracy, you may do so, by making the altitude AH as small as you please (a).

(a) Both in this and the former solution, pag. 102, it appears, that to find the air's rarity at any proposed altitude, it is necessary first to determine it by experiment at some one altitude. But the Author has improved these solutions in his *Harmonia Mensurarum*, so as to find the rarity at any proposed altitude without that determination; which may be looked upon as no small curiosity, and also as a particular proof, among many more in that admirable book, of the great excellency of the Author's general method of reducing problems to the Measures of Ratios and Angles, and thence immediately to the tables of Logarithms, Sines and Tangents; as being the shortest and easiest way, both in theory and practice, to an actual solution. As an instance of this I will give his solutions of the problem before us, in the form of the following Rules for computation, omitting the geometrical representation of them as requiring

LECTURE XV.

Air is the medium that propagates sounds: their velocity and manner of propagation.

FROM the experiments we have been making it is very easy to infer this conclusion, that the air is that medium by which all sounds are propagated from sonorous bodies and conveyed to our ears. As the air was in part exhausted from our receiver, so was the sound of the bell proportionably diminished; and when we seemed to have made an almost perfect evacuation, the sound also at the same time seemed almost entirely to cease. On the other hand as the quantity of air was augmented by the condenser, the sound received a like augmentation, and as the augmentation of the air became yet greater, so did that of the sound also, and seemingly in the same measure. It is therefore highly reasonable to

ing a previous knowledge of the Author's definitions of the measures abovementioned.

Putting l for the logarithm of the rarity required at any proposed altitude a , m for the constant logarithm 0.4342944819, b for the height of an homogeneous atmosphere, reduced every where to the density of the air we breath, and s for the semidiameter of the earth; upon the hypothesis of an uniform gravity of the air at all altitudes, we have $l = \frac{m}{b} \times a$; but upon the true hypothesis of a decreasing gravity, as the square of the distance from the center increases, we have $l = \frac{m}{b} \times \frac{s}{s+a} \times a$.

In the same book the Author has also given us a general solution of this problem, as simple as these particular ones, supposing gravity to be as any given power of the distance from the center.

The Author's estimate of the height b in page 97, is 29254 feet; Sir Isaac Newton makes it 29725 feet, taking his quicksilver to water as $13\frac{2}{3}$ to 1, and water to air as 870 to 1, when that quicksilver is 30 inches high in the barometer. *Phil. Princip. lib. 2. prop. 50. Schol.*

conclude,

conclude, that the air is the true and only medium by which all sounds are propagated from one place to another; for the cause must answer to its effect, and the effect to its cause; as the one is augmented or diminished, so must the other necessarily be augmented or diminished in the same proportion.

But it may perhaps be said, that the air concurs only to the production of sounds and not to their propagation. For possibly by the absence of the air, the sonorous body may undergo such a change in its parts, as to be rendered incapable of being put into those motions which are absolutely requisite to excite in us the sensation of sound. This, I say, may perhaps be objected to us, and if we admit the objection, it must be confessed that our experiments do not prove so much as was intended to be collected from them. For my part I must acknowledge that the supposition upon which this objection proceeds, is to me altogether inconceivable; for according to my apprehensions of the matter, the presence of the air should rather obstruct and in time destroy those tremulous motions of the sonorous body, than any way contribute to their production. However, for your further satisfaction, I shall here add an account of an experiment made some years ago by Mr. *Hauksbee*, and published in the *Philosophical Transactions*; by which it will appear that sounds actually produced, cannot be transmitted through a *vacuum*.

“ I took, says he, a strong receiver armed with a
 “ brass hoop at the bottom, in which I included a
 “ bell as large as it could well contain. This re-
 “ ceiver I screwed strongly down to a brass plate with
 “ a wet leather between, and it was full of common
 “ air, which could no ways make its escape. Thus
 “ secured it was set on the pump, where it was co-
 “ vered with another large receiver. In this manner
 “ the air contained between the outward and inward
 “ receivers

“ receivers was exhausted. Now here I was sure,
“ when the clapper should be made to strike the
“ bell, there would be actually sound produced in
“ the inward receiver; the air in which was of the
“ same density with common air, and could suffer no
“ alteration by the *vacuum* on its outside, so strongly
“ ly was it secured on all parts. Thus all being ready
“ for trial, the clapper was made to strike the
“ the bell; but I found that there was no transmission
“ of it through the *vacuum*, though I was sure
“ there was actual sound produced in the inward receiver.”
And from this experiment he very justly concludes, that air is the only medium for the propagation of sounds.

Now to make us understand the manner by which this propagation is performed, philosophers have generally had recourse to that very obvious instance of a stone, or any other heavy body, thrown into a pond of stagnating water. For as the surface of the water forms itself into circular waves, which are successively propagated from the stone as from a center, and are continually dilated in their progress, becoming still greater and greater as they are further removed from the center, till at last they reach the banks of the water and there vanish, or dashing themselves against it are reflected back again; so they tell us, that the tremulous motion of bodies which is requisite for the production of sounds, does excite in the air the like undulations, which are also propagated to very great distances in successive rings, every way encompassing the sonorous body; and these undulations meeting with our organs of hearing, impress upon them a certain tremor, which does necessarily excite in our minds the sensation of sound.

It must be allowed, that this example is proper enough to illustrate and represent to us those invisible motions of the air, by which the conveyance of sounds

sounds is made from one place to another. But the comparison ought not to be carried on too far; for it is very certain it will not hold good in every respect, and some philosophers of great note, have overshot the mark by endeavouring to make out a more exact correspondence than was needful.

I shall here take notice only of two particulars, in one of which an agreement may be observed in these motions of air and water; in the other a disagreement. It is easy to perceive upon the surface of such a pond as I have been speaking of, that the watry undulations are propagated not only directly forwards, but if any obstacle happen to be placed in their way so as to obstruct their progress, they will bend their course about the sides of the obstacle, and dilate themselves by an oblique motion, into that part of the pond which lies immediately behind the obstacle; which part of the surface of the pond must have remained perfectly smooth, and could never have participated of this undulating motion, if it were not otherways propagated than by straight lines proceeding directly from the central body which first excited it. Imagine a partition to be drawn cross the pond, from the one side of it to the other, by which it may be divided into two parts, and in the middle of this partition conceive a small aperture to be made, by which the water on the one side of the partition, may have a communication with that on the other side of it. Then if the water on either side be put into an undulating motion, the waves will continue to extend themselves till they arrive at the partition, and there will in part be reflected back again, and in part be permitted to pass through the aperture; and after their passage you will perceive them to be regularly dilated from the aperture as from a center, and to spread themselves over the whole surface of the pond which lies behind the partition; not by a direct

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rect motion from the place in which they were at first excited, for this cannot be by reason of the partition, but by an oblique and lateral progress, their course being bent as they pass through the aperture.

In the same manner it may be observed, that those undulations of the air by which sounds are conveyed from place to place, are not only propagated in straight lines, proceeding directly from the sonorous body, but if any obstacle happen to be interposed, they also bend their course about the obstacle, and arrive at the ears of the hearer by an oblique motion. Thus two persons may very well hold a discourse with each other, though a very high wall be between them, and it is certain the sound in this case is not carried from the one to the other by a direct motion, but after it has ascended from the speaker to the top of the wall, its course is there bent, and so it proceeds down again to the hearer.

Thus if a gun be discharged on the one side of a mountain, we may easily hear the sound of it on the other side; though it be very certain that this sound could never reach our ears unless it were propagated obliquely over the top of the mountain, or by the sides of it. That these things are matter of fact every body's own observation will convince him. But philosophers are sometimes led on by prejudice, to argue even against matter of fact. That we may therefore oppose them in their own way, it will be worth our while to examine a little into the nature of the thing, and to see what conclusion we may possibly infer from thence.

In order to this I shall in the first place endeavour represent to you, as clearly and distinctly as I can, the manner by which these undulations are produced in the air and continued; for those upon the surface of water need not here be any further insisted on, their oblique propagation seeming indeed never to have been called into question. We

We are therefore to understand, that the parts of the sonorous body, being put into a tremulous and vibrating motion, are by turns moved forwards and backwards. Now as they go forwards they must of necessity press upon the parts of the air to which they are contiguous, and force them also to move forwards in the same direction with themselves; and consequently those contiguous parts will at that time be condensed; then as the parts of the sonorous body return back again, the parts of the air which were just before condensed, will be permitted to return with them, and by returning they will again expand themselves. It is manifest therefore, that the contiguous parts of the air will go forwards and backwards by turns, and be subject to the like vibrating motion with the parts of the sonorous body.

And as the sonorous body produces a vibrating motion in the contiguous parts of the air, so will these parts thus agitated, in like manner produce a vibrating motion in the next parts, and those in the next, and so on continually. And as the first parts were condensed in their progress and relaxed in their regress, so will the other parts, as often as they go forwards, be condensed, and as often as they go backwards, be relaxed. And therefore they will not all go forwards together and all go backwards together; for then their respective distances would always be the same, and consequently they could not be rarefied and condensed by turns; but meeting each other when they are condensed, and going from each other when they are rarefied, they must necessarily one part of them go forwards whilst the other goes backwards, by alternate changes from the first to the last.

Now the parts which go forwards, and by going forwards are condensed, constitute those pulses which strike upon our organs of hearing and other obstacles

cles they meet with; and therefore a succession of pulses will be propagated from the sonorous body. And because the vibrations of the sonorous body follow each other at equal intervals of time, the pulses which are excited by those several vibrations, will also succeed each other at the same equal intervals. You see then that the undulations of the air consist in a successive and interchangable rarefaction and condensation of its several parts, as those of water consisted of successive and interchangable ascents and descents of the several parts of the water: the pulses or denser parts of the air correspond to the ascents of the water, and as those elevated parts of the water descend again by the force of their gravity, so these denser parts of the air expand themselves again by the force of their elasticity.

This further may be observed, that though the pulses are carried on to very great distances by a direct progressive motion, yet the spaces in which the parts of the air perform their vibrations, may be very small and inconsiderable. To propagate the pulses, it is not requisite that the whole body of the air should be moved on directly forwards as in the case of winds; for by such a motion, as was said above, the several parts of the air would always retain their respective distances from each other, and consequently they could not be successively and interchangably rarefied and condensed, and thereby no pulse would be made. It is therefore absolutely necessary that they move backwards and forwards by turns; and how small soever the space is in which their vibrations are performed, it will be sufficient to cause a successive condensation of the parts; in which successive condensation the progress of each pulse consists.

It now remains to be proved that these pulses and undulations are propagated not only directly forwards, according to the tendency of the motion of
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the parts of the sonorous body, but sideways also, spreading themselves obliquely into the neighbouring regions of the air, which would otherwise remain at rest, as lying out of the tract of their direct motion. This neighbouring air, which borders on the sides or edges of that tract, being in its natural state of expansion, will be rarer than the air which makes the pulses, and denser than that in the intervals between the pulses. It must therefore necessarily come to pass, that the air of the pulses will expand itself laterally into those parts of the bordering spaces which are over against the pulses, and those parts of the bordering air, which are over against the intervals of the pulses, will for the same reason dilate themselves laterally into the intervals; and thus the bordering air will become denser over against the pulses, and rarer over against the intervals, and so partake of that undulating motion which was at first directly propagated from the sonorous body.

And as this air which immediately borders upon the tract of direct motion, owes its undulations to the air contained in that tract, so does it after the same manner communicate the like undulations to the air which is next to it on the other side, and that communicates the like to the next, and so on continually. And thus the undulations are propagated into all the neighbouring regions; not always indeed by a motion proceeding in straight lines from the sonorous body, but if those regions happen to lie out of the tract of direct motion, or to be placed behind some obstacle, the propagation is made by such a lateral and oblique diffusion as has been above described. It is evident therefore from the consideration of their nature, as well as from experiment and matter of fact, that these aery undulations agree with those of water in being propagated not only directly forwards, but also obliquely, so as thereby to pervade

pervade the spaces which lie behind any obstacle which may be opposed to their progress. And the same may be said of any other sort of motion or pressure which is conveyed by the intervention of any fluid. And this was the first of those two particulars which I thought fit to take notice of.

If the account I have been giving of this matter, from the philosophy of Sir *Isaac Newton*, may any way contribute to a clearer conception of the manner by which sounds are transmitted through the air, it will be no loss of time to have insisted so long upon it. But besides this advantage, I had further in my view the decision of a notable question among philosophers, concerning the propagation of light. Some have thought that the body of the sun does all around it, incessantly cast out from itself, with an almost incredible swiftness, those very fine and delicate particles of matter, which after they have traversed so vast a distance, impress upon our organs of seeing that peculiar motion which is requisite to excite in our minds the sensation of light. Others on the contrary, and those the more numerous, have believed that the lucifick particles which immediately affect our sense of seeing, are by no means themselves derived from the body of the sun, but their motion only; and this motion they imagine to be communicated to them from the sun, by the mediation of a very subtle and ætherial fluid, whereof they themselves make a part.

Des Cartes indeed who held a *plenum*, thought it sufficient to make the action of light consist in a pressure only, which he conceived to be instantaneously conveyed from the sun by means of that *plenum*. But this notion was afterwards confuted by a surprising discovery of Mr. *Roemer*, who made it evident from observations of the eclipses of Jupiter's satellites, that the propagation of light was not instantaneous,

stantaneous, though its swiftness was found to be almost beyond comprehension, as requiring no longer a time than that of half a quarter of an hour to pass from the sun to the earth, which space a cannon ball would be 25 years in describing.

Hereupon *Hugenius* proposed a new hypothesis, in which he supposes this very swift, but not instantaneous, motion to be propagated from the sun by successive undulations of the æther, in all respects alike to those of the air, by which the motion of sounds is transmitted from sonorous bodies. When we take a particular view of the several parts of this hypothesis, it appears to be so very ingeniously contrived, and so handsomely put together, that one can hardly forbear to wish it were true. But it is very manifest, from the observation I have lately been making, that neither this nor any other hypothesis can be so, which supposes the progress of light to depend only upon the agitations of a fluid medium, conveyed successively from the luminous body to our senses. For if any such hypothesis were true, the consequence of it would be this, that light would not only go directly forwards in straight lines from the luminous body, but might also diffuse itself obliquely, after the manner of sounds, when any obstacle happened to be interposed, and so it might dilate itself into the spaces which lie behind the obstacle; and thus we should have a perpetual day even at midnight, and a total eclipse of the sun's light would be a thing altogether impossible. I might here add some further considerations concerning the motion of light, but since I cannot do it without making this digression too long, I shall omit it and return to the subject I have undertaken.

The second particular then, which I proposed to take notice of, was concerning a certain disagreement of the undulations of air and those upon the

surface of water; I mean in respect of the velocity with which they are moved forwards. It has been found by many repeated observations, and is generally agreed on, that all sounds are transmitted through the air with one certain, determinate velocity; the greatness or smallness of the sound not in the least contributing to the acceleration or retardation of its motion. Now it might be expected that the undulations of water should also, in like manner, spread themselves from the central body, which excited them, how different soever the weight, magnitude and force of that central body might happen to be, with one fixed and determinate velocity, though that velocity were different from the velocity of sounds, as being propagated through a different medium.

Gassendus indeed, who imagined a perfect correspondence between these waves of water and those of the air, by which sounds are conveyed, was of opinion that the matter stood thus; but his opinion has justly been censured by the famous Florentine Academy del Cimento, who found it would not answer upon trial. They tell us on the contrary they have observed by frequent experiments, that by how much the stone is larger and the force greater, wherewith it is thrown into the water, by so much the circles approach the shore swifter.

Sir *Isaac Newton* in his excellent *Principia Philosophiæ* has carried this matter somewhat further. He considers the nature of these waves and the manner by which they are formed; and from that consideration he determines their velocity *a priori*. His conclusion is this, that as these circles are excited by a greater force, and consequently their distances from each other become greater, their velocities will be increased in a subduplicate proportion of their distances. Thus if the force be increased so much, that the distances of the waves become four times as great

as before, their velocities will be double; if the distances become nine times as great, the velocity will be triple; if the distances are sixteen times as great, the velocity will be quadruple, and so on. And more particularly he shews that the velocity in all cases is such, that if the length of a pendulum be taken equal to the distance of the waves, those waves will describe a space equal to their distance whilst that pendulum performs its vibration. And therefore if the distance of the waves be 39.2 inches, they will describe that space in a second of time; but if the distance be greater or less than 39.2 inches, the space described in a second of time will be augmented or diminished in the subduplicate proportion of that by which the distance is augmented or diminished.

His manner of making out these conclusions is very curious, and I believe I might render it sufficiently intelligible; but since it is not absolutely necessary to my purpose, I rather chuse to refer you to his book. That which more immediately belongs to our present consideration, is the velocity of sounds, and this is also determined *a priori*, from its causes, by the same incomparable philosopher. The train of reasoning which he makes use of on this occasion is so wonderfully subtle and requires so very close an attention, that I think it not proper in this place to enter into every particular of it; and it may perhaps be sufficient to understand in general the method by which he proceeds.

You have already seen, that in order to form those undulations which are requisite for the conveyance of sounds, every particle of the air must be moved forwards and backwards by turns, within a certain very small space. He goes on yet further, and shews that this progressive and regressive motion is not uniform, but is by degrees accelerated and retarded; and in particular that the laws by which this acceleration

and retardation is regulated, are exactly the same with those to which the motion of a pendulum is subject.

This being demonstrated, he conceives the atmosphere to be reduced to such an uniform state, that its density in every part of it may be the same with the density of the air at the surface of the earth. The height of the atmosphere so reduced, you remember, was formerly proved to be about $5\frac{1}{2}$ miles. To this height he imagines a pendulum to be equal in length, and then making his enquiry concerning the proportion of the times in which the airy particles and that pendulum perform their respective vibrations, by comparing the spaces described and the forces with which they are described together, he finds that the time of the particles of air, is to the time of the pendulum, as the distance of the waves of air from each other, or the latitude of the pulses, is to the circumference of a circle whose semidiameter is the length of the pendulum, or that height of the atmosphere which was before mentioned.

Now the pulses by going forward describe a space equal to their latitude in the time that each particle of the air performs its course.

Therefore the time in which the pulses describe their own latitude, is to the time in which the pendulum performs its vibration, going forwards and returning back again, as the latitude of the pulses, is to the abovementioned circumference.

And from hence he deduces this conclusion ; that the velocity of these aery pulses, or which is the same thing, the velocity of sounds, is of such a quantity, as to describe a space equal to the circumference of a circle whose semidiameter is the height of the atmosphere, in the time that a pendulum, whose length is the same with that height, performs its vibration, by going forwards and returning back again. Or to express the same thing by a somewhat differ-

different but easier manner, that the velocity of sounds is equal to the velocity acquired by an heavy body in falling from half that height of the atmosphere, supposed to be of the same uniform density in all its parts.

From this conclusion he proceeds to his computation, and after all due allowances are made, he finds that the number of feet which sounds describe in a second of time, is 1142; which agrees with the most exact observations. And this again is another full and perfect proof, that the air alone, and not any other more subtile fluid, which may be imagined to be interspersed through the body of it, is the proper vehicle of sounds.

It must indeed be confessed, that those who have observed the motion of sounds, have not always agreed in their measures; but then their disagreement is to be ascribed to a want of exactness in the methods they used, or to the smallness of the distances at which their trials were made. I shall here give a relation of some observations which may best be depended on.

Cassini, *Picard* and *Roemer*, three excellent members of the French Academy of Sciences, made their experiment at the distance of about a mile and an half, and found that the space described in a second of time was 1172 feet. The Florentine Academy del Cimento made their trial at the distance of about 3 miles, and found that the space described in a second of time was 1148 feet. *Dr. Halley* and *Mr. Flamsteed*, by an observation made at the same distance concluded upon 1142 feet. And this last determination is confirmed by the most exact enquiries of the Reverend *Mr. Derham* Rector of Upminster in Essex, and Fellow of the Royal Society, who has lately published in the Philosophical Transactions, a particular treatise upon this subject; giving an account

of several observations made by himself with the utmost care and diligence, for the space of three years together, at various distances from one mile to more than twelve. We may therefore very safely conclude, that the velocity of sounds is of such a quantity, as to describe very nearly 1142 feet in a second of time.

I say very nearly, because it is certain this velocity may be a little augmented or diminished by favouring or contrary winds, and by heat or cold, notwithstanding what some philosophers have said to the contrary. It is very well known, that winds are nothing else but a body of the air moved forwards, with a direct progressive motion. If therefore that body of air be moved the same way with the pulses of sound contained in it, the pulses by participating of that motion, will be accelerated, if the contrary way, the pulses will be retarded; so that the velocity of the sound will in the former case be augmented, in the latter be diminished, just so much as the velocity of the wind amounts to.

The Florentine Academy and some others, who have designedly made experiments for this purpose, have not been able to observe that winds had any sensible influence upon the velocity of sounds; and thence it came to be generally believed, that there was not any the least acceleration or retardation upon that score. But Mr. *Derham* has at length undeceived us. He assures us, that by many certain observations he has found an alteration of swiftness, which though it be small, is yet sufficiently sensible in those very large distances at which he made his trials. He tells us he has also made many experiments concerning the velocity of winds, and in particular he says, that a storm so exceedingly violent as almost to overturn a windmill, which stood near the place where he made his observation, was found by many repeated trials to move not above 66 feet in a second.

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Whence it is easy to understand that more moderate winds can cause but a very small change in the velocity of sounds.

Let us now go on to consider the effect of heat and cold. Since by heat the air contiguous to the surface of the earth is expanded, it is manifest that the height of the atmosphere, supposed to be every where of the same density with this contiguous air, will be increased in proportion to that expansion; and therefore the velocity of sounds, which is equal to the velocity acquired by an heavy body in falling from half that height, will be increased in a subduplicate proportion of the same expansion. And the like may be said as to the effect of cold, namely, that the velocity of sounds will thereby be diminished in a subduplicate proportion of the air's contraction.

Hence from some observations made upon the expansion and contraction of the air from its greatest degree of heat in our climate, to its greatest degree of cold, I find that the middle velocity of sounds may be increased or diminished about a thirtieth part of the whole; and by that means they may move about 38 feet more, or so much less than 1142 feet in a second of time, accordingly as the season is either hot or cold to an extremity (*a*).

Mr. *Derham* tells us he could never observe any

(*a*) By Mr. *Hauksbee's* experiments (Phil. Transf. N^o. 315.) the proportions of the greatest, middle and least expansions of common air in this climate, are expressed by these numbers, 144, 135, 126, which are as these numbers 32, 30, 28, as appears by dividing by 4,5. Supposing then the middle height of the uniform atmosphere to contain 30 equal parts, the greatest height will be 32 and the least 28 such parts. Therefore by what has been said above, the middle velocity of sounds will be increased in the subduplicate ratio of 30 to 32, that is, in the ratio of 30 to 31 very nearly; which increase is $\frac{1}{31}$ of the middle velocity, or $\frac{1}{31}$ of 1142 feet, or 38 feet in a second of time; and so much may the middle velocity be diminished.

change of velocity occasioned by heat or cold, but we ought not from thence to conclude, against the theory, that there is none. I am unwilling to question either his diligence or fidelity, and therefore I choose rather to say, that possibly at the times when he made his trials, the quality of the season might not be very intense, and consequently the change might be so small as to escape his observation; which may easily be admitted, since at the utmost it amounts but to the thirtieth part of the whole velocity.

What he further adds concerning the variation, of the height of the mercury in the barometer, namely, that this has no influence upon the motion of sounds, may be depended on with more security; for this is also confirmed by the theory.

It is certain that the height of the atmosphere, supposed to be reduced to the same state of density with the air we breath in here below, is not any ways altered upon those variations of the barometer. For though the quantity of that uniform atmosphere be often changed, yet setting aside the consideration of heat and cold, the density of it is always changed in the same proportion, and therefore the height of it does always remain unaltered; and consequently the velocity acquired by falling from half that height, which is equal to the velocity of sounds, does also remain unaltered.

Hence it is easy to understand, that the transmission of sounds is equally swift through a rarer or denser air, supposing the elasticity of it to be augmented or diminished in the same proportion with its density; which always comes to pass, excepting when that proportion is a little disturbed by heat or cold. I shall here conclude what I think sufficient to have been said concerning the propagation of sounds.

LECTURE XVI.

Air sometimes generated, sometimes consumed; the nature of factitious airs; explosions in vacuo, dissolutions, fermentations, &c.

THOUGH the experiments we have been making are sufficient to convince us, that air may in very great quantities be produced from bodies which suffer any considerable alteration in the texture of their parts, whether that alteration be made by almost insensible degrees, as in putrefactions and very slow fermentations, or whether it be made more swiftly, as in some dissolutions, or even almost instantaneously, as in the explosion of gunpowder; yet for the wonderfulness of it, I should have added one further trial made some time ago by Dr. *Slare*, had not the danger of it deterred me from attempting to repeat it. His own account of the experiment is as follows.

“ We took, says he, half a dram of the oyl of carui-seeds and poured it into a little gally-pot, and
 “ put a dram of our compound spirit of nitre, in a
 “ small vial, into the same gally-pot, and placed over
 “ it a glass that held three pints upon Mr. *Papin*’s
 “ exhausting engine; and having soon cleared it of
 “ the air, we turned up the vial in order to see what
 “ effect would ensue, in the *vacuum*, upon this mixture; but in the twinkling of an eye the receiver
 “ was blown up, and the mixture in a flame, which
 “ stupendous phænomenon surpris’d and frightened
 “ us all. Nor did I ever see or hear of the like by any
 “ mixtures made in *vacuo*, though I have my self
 “ seen a thousand. For if we look into those many
 “ admirable experiments made by the immortal Mr.
 “ *Boyle*, the removal of the air did almost always
 “ extinguish light and fire and flame. The blowing
 “ up

“ up of the glass does also make the experiment the
“ more astonishing, and puzzles one how to account
“ for so great a quantity of air as was produced from
“ these liquors, which amounted only to a dram and
“ an half; for here was required not only air enough
“ to fill up the capacity of the vessel, but also there
“ was required so great a pressure within, as did ex-
“ ceed that great incumbent weight of air that press-
“ ed upon this capacious glass without, whose dia-
“ meter was six inches and the depth above eight;
“ for otherwise it would not have thrown it up into
“ the air. If we review and consider well the phæ-
“ nomena of this experiment, we may find the re-
“ sistance of some hundred weight, that was coun-
“ terailed, and not only so, but with a much greater
“ force exploded. That it was not produced by any
“ expansion of the common air, for that was seen to
“ rise out of the liquors themselves and was drawn
“ out of them in their separate state by the exhaust-
“ ing engine, which suffers no elastical air to lie con-
“ cealed in any liquors. That it was produced in an
“ instant by the mutual collision and agitation of these
“ active and self-expanding liquors. That it was not
“ absolutely generated *de novo*, but that the air was
“ antecedently there, we may reasonably believe, al-
“ though in a very differing state from what it is in
“ when in *pleno*. For all that the exhausting engine
“ does, is to deliver the air from a state of compres-
“ sion, by leaving it to stretch itself like a bladder,
“ that has full liberty to swell up, and has no hard
“ body to straighten or oppose its expansion: so that
“ we have cause to conclude our liquors to be fur-
“ nished with this sort of air, which being by the
“ accension of these two liquors put to a new and
“ violent motion, does expand itself *de novo*, and
“ to that degree as to answer so great an effect as is
“ above-mentioned. The circumstances of which
phæno-

“ phænomenon will allow me to call this mixture a
“ sort of liquid gun-powder”. Thus far Dr. *Slare*.

I shall now proceed to give you an account of those curious and useful observations of Mr. *Boyle*, which make up the second Continuation of his *Physico-Mechanical Experiments*. These being the best and almost only trials which have yet been made concerning factitious airs, are very proper to give us what further light may be had concerning their nature and properties. I shall endeavour to make my extract as short as conveniently I can, that it may not be too tedious. Those who are desirous to see the particulars of each experiment, may consult the book itself at their leisure with greater advantage.

ARTICLE I. *Several ways used to help the production of air.*

Bread by itself does not readily produce any air in *vacuo*, but being very much moistened and a little kneaded, it yields a sufficient quantity; and thence it was concluded, that water is a fit dissolvent to draw forth air out of bread. The experiment was also tried by burning it in *vacuo* with a burning-glass, and by this means much air was generated, which did even and anon break out as by fulmination. And from these trials it was thought probable, that the air contained in bread is so closely confined, that no easy operation can give it a full discharge; but if any thing could dissolve and loose that knot, it might then produce great effects.

Dried grapes bruised and put into water, being included in a receiver produced much more air than others without water. It appears therefore, that water is a fit medium to elicit air out of them; but it was observed, that the production of air did not begin presently upon the affusion of water, but that it proceeded on with greater swiftness after the parts
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of the water in five or six days time had more deeply sunk into and pervaded the grapes.

Pears were included in two different receivers in *vacuo*, and it was found that in one of them, which was exposed to the rays of the sun, much more air was generated in the same time than in the other. Whence it was conjectured, that the production of air is very much promoted by the heat of the sun.

Bruised grapes in *vacuo* with spirit of wine produced more air than without that spirit. Whence it appeared, that spirit of wine doth advance the production of air from bodies included in *vacuo*; though by other experiments it appears wholly to hinder that production from bodies included in common air.

From experiments made with apples both boiled and raw, both with sugar and without it, in larger and smaller receivers, it was concluded that sugar, the crudity of the fruit, and the largeness of the receiver do all contribute to the production of air.

ART. II. *Several ways to hinder the production of air.*

Paste made with bread-corn-meal, without leaven, was put into an empty receiver, and afterwards the receiver was placed in a certain apartment with a good fire in it, yet the paste produced no air in ten hours space. Hence it was thought, that if paste hath once suffered too much cold, it can scarce recover its faculty of fermenting. For at another time paste made without leaven in the summer season, produced very much air in a short time in *vacuo*.

Dough kneaded with leaven had a quantity of spirit of wine poured upon it, to try whether fermentation would be hindered by that means, and it was concluded by the event, that the spirit did hinder the production of air.

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By some trials upon pears it was collected, that fruits included in a receiver, with very much compressed air in it, cannot produce so great a quantity of factitious air as in a medium less dense. It was also further collected that artificial air is sometimes produced by iterated turns, and as it were by reciprocations, and that the changes of heat and cold, though they are not the sole causes of such reciprocations, yet seem to contribute much thereunto.

The same things were concluded from experiments made with paste. Raisins of the sun steeped in vinegar, were placed in an emptied receiver, and it was thereby found, that vinegar was an hindrance to fermentation and the production of air.

Plums and apricots, many of them being cut asunder, were placed in two receivers, in one of which was air produced from cherries, and in the other common air; and it was found by this experiment, that the artificial air of cherries was a great hindrance to the apricots, that they could not produce air; yet notwithstanding, it was thought to advance the alteration of their colour and firmness, and to be good to preserve their taste.

Grapes included in common air with spirit of wine and without it, shewed that in common air the spirit of wine doth hinder fermentation; though by other experiments it was found to promote it in *vacuo*.

Some peaches were included in an exhausted receiver, and together with them some spirit of wine, which could not touch the peaches unless it were elevated in the form of vapours. A like quantity of peaches was also included in another unexhausted receiver without spirit of wine. And it was found by this experiment, that the very vapours of spirit of wine, do somewhat hinder fermentation and the production of air; but much less than the spirit itself.

From experiments made upon paste with leaven
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and without it, with spirit of wine and without it, it was concluded, that leaven doth rather hinder than help the production of air, if the paste be not made in a place hot enough; and that spirit of wine doth very much prejudice the production of air, and the rather if the paste be wrought with the ferment; and moreover that paste without ferment in tract of time will produce no less air than paste with ferment.

New ale was included in a receiver exactly filled that so no air might be left, and another quantity of the same ale was included in another receiver wherein some room was allowed for the air; and from the experiment it seemed to follow, that ale if the air be excluded from the vessel, will ferment more slowly than if some air were left in; yet in tract of time, it makes a greater compression if no place be left for its dilatation.

Green pease in an emptied receiver with spirit of wine and without it, shewed that spirit of wine doth hinder the production of air from pease.

ART. III. *The effects of artificial air are different from the effects of common air.*

From two experiments made upon cherries it was concluded, that in artificial air fruits do produce less air, and so they keep their colour and their taste better: it was also observed, that cherries do contain much air in them, and that they produce it very irregularly.

A trial was made with unripe grapes in common air and in factitious air produced from pears, and it was from thence concluded that factitious air was fit to alter the colour of fruit and to preserve its taste.

A mixture of common air and air produced from cherries, was found to preserve oranges better than common air alone.

Two pieces of beef were placed in different receivers,

ceivers, in one of which was common air, in the other cherry-air; and it was concluded from the comparison of the events, that cherry-air is a great hindrance to the production of air from flesh.

Two onions were put into a receiver full of common air, to see whether vegetation would increase the quantity of air or diminish it. Two other onions were put into a receiver with air produced from paste; and it was gathered from the event, that artificial air doth not at all hinder vegetation, and that not only the sensible bigness of the body, but also the quantity of air is increased by vegetation.

Unripe grapes were included in common air and in factitious air of pears; and the comparison confirmed the efficacy of artificial air to alter the colour of fruits. But it was observed in this experiment, that it prejudiced the preservation of taste, and promoted the production of air, contrary to what had happened in some of the former experiments.

Gilliflowers were included in three different receivers, one of which was exhausted, another had common air in it, and the third contained artificial air of paste; and it was observed that factitious air renders the change of colour more speedy, yet it prevents mouldiness even as a *vacuum* doth the same.

From an experiment made in two receivers with common air and with artificial air of cherries, it was found, that the alteration of colour and firmness in apricots is promoted by cherry-air, and that some part of such air is destroyed in the beginning.

Plums cut asunder were put in three different receivers, the one exhausted, another full of gooseberry-air, the third full of common air; and by this experiment artificial air seemed to have promoted alteration.

Some peaches were put into a receiver with common air mixed with air produced from grapes, and the grapes themselves were included in the same receiver, that the common air might be the better saturated with the artificial; and from the circumstances of this experiment it was concluded, that common air doth corrupt bodies, yet it doth so much less, if it be mixed with factitious air.

Equal parts of pears cut asunder were placed in four different receivers, one of which was full of common air closed up, another was full of common air but not exactly closed, another contained cherry air, and the last was evacuated. It was observed, that corruption doth not begin in free air sooner than in included air; but when it is begun, it is much more increased and more speedily, the included air seeming to be sooner satiated. The aptitude of artificial air for the softning of fruits was also observed. And it seemed probable, that the production of air was here promoted by the artificial air, though it had succeeded otherwise with apricots in the other trials.

Apricots were included in four different receivers, one had common air and was closed, another had common air but was left open, the third had a mixture of air produced from pears, the fourth contained common air but pretty much compressed. It was from hence concluded, that the quantity of corruption doth depend on the quantity of air, and also that in factitious air alteration is made quicker, but in tract of time the corruption is far greater in common air.

ART. IV. *The effects of compressed air are different from the effects of common air.*

Onions set to grow in common and condensed air, shewed, that a little compression doth not prejudice those

those bodies which are to be expanded by vegetation.

Tulips and lark-spurs placed in common and condensed air shewed, that compression in some plants doth hinder putrefaction and moulding.

The two halves of an orange were included in compressed and common air; and it was confirmed by this experiment, that compressed air may somewhat retard corruption, yet in progress of time it was made probable by other experiments, that the quantity of corruption doth depend upon the quantity of air.

Equal quantities of roses were included in common and compressed air; and from hence it seemed to follow, that compressed air is something fitter for the alteration of colour than common air.

The two halves of an orange were again included in common and compressed air; and from the circumstances of the experiment it was concluded, that the quantity of mouldiness doth depend on the quantity of air.

Two mice were included in common and compressed air; and it was found that the mouse in the common air had consumed something of that air. By comparing the times they each lived under their confinement, it was judged that compressed air was fitter than common air for the prolongation of life; but it is to be observed that this compression was not very great.

The experiment was tried with flies, and they seemed not to be sensible of a small compression, nor indeed are they much prejudiced by a rarefaction of the air, unless there be an almost compleat *vacuum*.

The experiment was made also with frogs, but nothing could with certainty be concluded from it.

Another trial like the former was made with a dissected orange, and it was again confirmed, that

the quantity of mouldiness doth depend on the quantity of air. But it was observed, that the mouldiness did appear a little later in the compressed air than in the common, though afterwards it increased much more.

The like was again concluded by experiments made upon roses, the parts of a limon and upon gilliflowers.

A mouse was put into a receiver with common air, only to try whether he would produce or consume air. From the event it was concluded, that living animals consume air, but dead ones produce it.

Pears of the same sort were included in compressed air, in common air and in *vacuo*; and it seemed to follow from the event, that in a great compression a less quantity of air is produced.

From some other experiments made upon animals, it was found that a very great compression of air is noxious and even mortiferous to them.

ART. V. *The effects of artificial air upon animals.*

A bee, with distilled vinegar and pulverized coral, were put into a receiver, and the air being wholly exhausted, matters were so ordered that the coral fell down into the glass of vinegar. But the air produced from thence, did not restore any power of motion to the bee; but when she was exposed to the open air, in a little time she began to move herself. Hence it was suspected that artificial air is unfit for the life of animals.

Two flies were included in a receiver out of which the common air being exhausted, some gooseberry-air was made to supply its place. Afterwards two other flies were included in *vacuo*, but with this difference, that common air was restored to these latter flies. The event was, that the latter flies recovered

vered thereby their power of motion, which they had lost in *vacuo*; but the former in the factitious air remained irrecoverably dead. The experiment was repeated with the same success; and this was thought to be an high confirmation that artificial air is noxious to the life of animals.

Three receivers being filled with air produced from paste, a perfumed cone was kindled and put into one of them, which being stopped, the fire within a minute of time went out. Then by blowing with a pair of bellows, the artificial air was expelled from the receiver, and the fire was again put into it as before, and now it burned bright for a pretty long time, though the receiver was shut as speedily and as accurately as before. Into the second receiver a fly was put, and presently seemed to be dead, but being exposed to the sun, she recovered again in a short time. Then common air being blown into the receiver, the fly included as before suffered no inconvenience thereby. The same experiment was tried with the same fly in the third receiver, being filled with artificial air, and the same success followed, excepting that the fly being now longer included, could not so soon as before be recovered to health again. Hence it appeared that artificial air is not only prejudicial to the life of animals, but to flame also.

Several other such experiments were tried with various animals, from whence it was concluded as before, that factitious air is very hurtful to their life; but if mixed with common air it doth not so readily produce its effects; it appearing to be so much the more hurtful as it is the more free from that mixture. It was also made evident, that factitious air is a greater enemy to animals than a *vacuum* itself, and thence it was collected that it kills by some venomous quality, and not only by the defect of com-

mon air. Air produced from cherries was found to be somewhat less hurtful to frogs than that produced from paste; air produced from gooseberries less hurtful to mice than air produced from gun-powder; air produced from pease less hurtful to snails than air produced from paste.

ART. VI. *Animals in vacuo.*

A butterfly being put into an emptied receiver was almost three hours before it was deprived of its faculty of motion. Then the air being let in, it recovered itself again. After this it was bound by one of its horns with a thread, and so it was suspended in the receiver, and it was carried very freely from one part of it to another by clapping its wings; but after the air was extracted again, the clapping of its wings was in vain, for it could not move the thread in the least from being perpendicular.

By an experiment made upon flies in very much rarefied air, it was concluded that a small quantity of air may suffice for insects to breath in.

Snails were included so long in *vacuo* till they seemed to have lost all power of motion, and in that state they produced some air, though they were not so perfectly dead as to be past recovery.

Fly-blowings or the eggs of flies were placed under a receiver in air much rarefied, and it was found by the event, that insects may be produced and may live, if not in *vacuo*, yet at least in highly rarefied air.

By another experiment of the like nature it was concluded, that insects could not be generated and live in *vacuo*, though they might in rarefied air; which thing was also confirmed by a farther experiment.

Vinegar

Vinegar full of those very small eels which may be discovered in it by microscopes, was for some-time included in *vacuo*, and another part of the same vinegar was kept in the open air; the eels which had been kept in *vacuo* were all found to be dead, though the others in the open air were as brisk as at first. Hence it was evident, that even those very diminutive animals are also affected with the presence and absence of the air.

ART. VII. *Contains some experiments concerning the consumption of fuel by fire in compressed air.*

It was concluded from those experiments, that the quantity of matter consumed in a given space of time did nearly answer to the quantity of compressed air.

ART. VIII. *Fire used to produce air.*

Paper besmeared with sulphur was burnt in *vacuo*, and some air was thereby produced, which was not at all diminished for two whole days. This air was ascribed to the paper, for it was found by other trials, that no air is produced out of sulphur alone.

Some air was produced from harts-horn burnt in *vacuo*, but part of this air was in a short time destroyed again, and the other part which preserved its elasticity for a full hour after the burning-glass was removed, seemed afterwards not to lose it at all.

Amber produced no air even by being burnt.

Camphire in *vacuo* was placed over a digesting furnace, and though it was sublimated into flowers, yet no air was produced.

Sulphur vivum was melted in *vacuo* by a burning-glass; but the fumes of it did not appear to contain any air.

Paste that had been included in *vacuo* for nine days and seemed to have emitted all its air, was endeavoured to be fired with a burning-glass. The subsiding fumes had tinged the surface of the paste with a curious yellow colour, and it was conjectured that some air was produced.

ART. IX. *Concerning the production of air in vacuo.*

Dried grapes and dried figs were placed in *vacuo*, and it was concluded from the event, that dried fruits in *vacuo* produce very little air.

Apricots appeared to produce their air almost as easily in their wonted pressure as in *vacuo*.

By comparing the events of cherries in *vacuo*, when whole and when dissected, it was concluded, that some dissected fruits do sooner produce their air than whole and undivided ones.

Cabbages cut in pieces were put into an emptied receiver, and it was thought from the circumstances of the experiment, that bodies when they putrify, have already produced almost all their air.

The same was confirmed by another experiment made upon apples.

Two equal quantities of milk were put into two glass receivers of equal bigness; the one was left in the free air, the other was evacuated. And it was observed, first, that the coagulation of milk, when the air is extracted therefrom, is somewhat retarded. Secondly, that the butter, whey and cheese are mixed with one another confusedly in the air, but in *vacuo* they keep their distinct places, and one swims upon the top of the other. Thirdly, that the putrefaction of milk is hindered or very much retarded in *vacuo*. Lastly, that milk by long continuance in *vacuo*, is made unfit to generate worms, even in common air.

A like experiment was made with urine. By comparing the quantity of air produced in this experiment, with that produced in the former, it seemed, that urine, which is an excrementitious humour, contains less air in it than milk which is an alimental humour. And moreover the efficacy of the air to corrupt urine was here very observable.

Paste very much diluted and without leaven, being put into a glass vessel, was placed in an emptied receiver, and though the vessel which contained it were not half full before all the air was exhausted, yet the same day the paste had swollen above the brims of the vessel. The next day the paste continued to swell more and more, and was interspersed with many cavities. The third day the paste was much more tumid than before, and much air was generated from it. The fourth day in the morning the cover was found to be separated from the receiver by the force of the produced air, and some of the paste was spread above the edges of the receiver, yet its swelling was somewhat abated. In the afternoon its tumidness was much more abated, yet it took up twice more room than it did before it was put into the receiver. The taste of it was not acid, and it was thought that bread thus made was very light.

A quantity of beef was put into an exhausted receiver, and it was concluded from the event, that flesh, whilst it putrifies, doth produce much more air than before it putrifies, though the contrary was before observed of fruits.

From an experiment made upon goosberries it seemed to follow, that these fruits after they have produced all their air, admit very little alteration; as if that air itself were the cause of corruption.

By an experiment made upon dried plums it was confirmed, that dried fruits are very unfit to produce air.

A trial was made with nut-kernels, and it appeared, that air may without sensible putrefaction be produced from fruits even of an hard consistence.

ART. X. *Concerning the production of air above its wonted pressure.*

An experiment made with goosberries seemed to prove, that goosberries contain much air in them, which as soon as it is freed from the usual pressure, doth more readily break forth than when it is restrained by some ambient air, until the goosberries begin to be fermented; for then air is produced in a far larger quantity, even in a great compression.

An experiment made with paste seemed to prove, that air may be produced out of paste in compressed air as well as in *vacuo*.

Horse-beans contain much air, which they produce very irregularly, both in *vacuo* and under a moderate pressure.

Goosberries produce their air regularly enough unless something be extracted out of the receiver, for then they acquire strength to produce new air more speedily.

Grapes produce not all their air but in a long tract of time.

Pears seemed to produce their air, as it were by paroxysms or fits.

ART. XI. *Various experiments.*

Melted lead and melted tin produced no air in *vacuo*. It was observed by the way, that the surface of these melted metals which were included in a brass vessel, was concave in *vacuo* after concretion, though in the common air it be convex.

Water saturated with salt was placed in *vacuo*, to try whether it would there be converted into crystals, as is usual in the free air, but it was found it would not.

Air produced from gooseberries was put into an evacuated receiver furnished with a mercurial gage. It was found that in the space of half a year, no change was made in the height of the mercury, and consequently the spring of this factitious air was not altered in so long a time.

A vial capable of containing 7 ounces, 5 drams and 3 grains of water, was exhausted of its air and weighed; then the bladder which covered its orifice was pierced with a needle, and thus being filled with air again, it was found to be $4\frac{1}{2}$ grains heavier than before, whence it followed that water was about 800 times more ponderous than an equal bulk of air.

Aqua-fortis with fixt nitre were placed in a receiver, which being exhausted, the one was poured into the other, and much air was thereby produced.

Spirit of wine was found to be very sensibly condensed by a moderate degree of cold, but not at all by a very great compression.

Spirit of wine and oil of turpentine were cleansed of air, then a quantity of the spirit of wine being put into a glass, some drops of the oil of turpentine were superadded to it, which swimming upon the spirit, were whirled about as is usual by an odd motion. Afterwards the vessel was placed in an exhausted receiver, and though no ebullition was made, nor any bubbles appeared, yet the drops continued to be moved in *vacuo* as in the open air. Hence it seemed to follow, that the cause of the motion of the drops is not to be ascribed to the dissolution of them, for all dissolutions in *vacuo* are wont to produce bubbles,

A glass containing spirit of sal-armoniac and the filings of copper was placed in *vacuo*. In a month's time the blew colour given to the spirit by the copper was almost quite vanished, but upon the admission of the air it quickly returned.

A mixture of aqua-fortis and spirit of wine was distributed equally into three glasses, into which three equal pieces of iron were put. One of these vessels being included in *vacuo*, a great many ebullitions were made in it. After a quarter of an hour the vessel was taken out again, and the liquor was found to be black and turbid, whereas the other two vessels had their liquor not altered in colour, but only some black powder did appear at the bottom of them.

Spirit of sal-armoniac with filings of copper were again placed in *vacuo*, and after the spirit had ceased to emit any bubbles, the filings were mixed therewith, which caused many bubbles to break forth again; but they were so far from producing any air, that on the contrary they consumed that which was there before.

ART. XII. *Artificial air destroyed.*

Air produced from cherries was transmitted into a receiver full of common air. It was concluded from the event, that air produced from fruits, at the beginning is in part destroyed; but the rest can keep the form of air very long.

Sal-armoniac was put into a receiver with a sufficient quantity of oil of vitriol, then the air being exhausted, the salt was put into the oil, whereupon a great ebullition presently followed, and the mercury in the gage shewed a good quantity of air to be generated; but this by the same gage soon after appeared to be destroyed again. The experiment was repeated, and both the production and destruction

were

were slower than before. Afterwards oil of vitriol alone was put into a receiver, in which only a fifth part of air was left to, try whether the oil without sal-armoniac would diminish the elastical force of the air; but it fell out contrary, that the force of the air was increased. It was confirmed by these trials, that some artificial airs may be destroyed, but it was thought to deserve a further enquiry, why this destruction happens sometimes sooner and sometimes later.

ART. XIII. *Experiments concerning the different celerity of air produced in vacuo or in common air.*

From these experiments made with paste, the kernels of filberds, raisins of the sun and onions, it was concluded, that some bodies do more easily produce their air in *vacuo* than in common or rarefied air.

ART. XIV. *The difference betwixt whole or entire, and bruised fruits:*

Bruised pears did not produce air so soon as entire ones. The same was found to hold as to bruised apples and unripe grapes bruised; but ripe grapes bruised had the contrary effect. By another experiment upon apples it was concluded, that bruised fruits do produce less air in *vacuo* than sound ones, contrary to what happens in the common air. The reason whereof was thought to be this, that bruised fruits are very much rarefied in *vacuo*, and so the several principles of which they consist, cannot act upon one another; but unbruised fruits, by reason of the entireness of their ambient skin, undergo less rarefaction.

ART.

ART. XV. *Contains some experiments,*

By which it seemed, that the air at divers times is diversly affected; so that sometimes it hath a power to hinder corruption and sometimes to promote it; sometimes it readily produces mouldiness, at other times it is unfit for that purpose.

ART. XVI. *Contains experiments,*

By which it appears, that some bodies, even in vessels hermetically sealed, may lose part of their weight by being exposed to the beams of the sun concentrated with a burning-glass.

ART. XVII. *Of the preservation of bodies in compressed liquors.*

Many experiments are contained under this article; the conclusions made from them are as follow.

That the taste of some fruits may be preserved in an infusion of raisins of the sun, at least in vessels which are able to contain a great compression of the air.

That liquors may grow sour though no spirits have evaporated from them.

That fruits cannot be long kept in pulp of apples by reason of the great production of air.

That the juice of crude grapes cannot conveniently be used for the preservation of fruits, for the same reason.

That fermented liquors may be useful for the preservation of fruits, as being unfit to produce air.

That beer may be convenient for the preservation of flesh, especially if it be intruded by force into the receiver; but this compression is soon abated, because the air compressed in the same receiver is apt

to enter into and pervade the pores of the beer by degrees.

That water as well as beer may conduce to the preservation of flesh.

That fishes produce less air than flesh, and yet that they will be corrupted, though they be fortified against the air.

That butter may be kept a great while if it be defended from the contact of the external air.

That corruption may sometimes happen without production of air.

That even tender bird's flesh may be preserved long by the help of beer or ale.

That sugar is not so fit for the preservation of fruits as fermented liquors.

That milk may sometimes be used with good success for the preservation of flesh.

That butter melted and hot is not so successfully used for the preservation of flesh.

That flesh after it is boiled, may be kept long without prejudice, which is a great convenience at sea, so that perhaps there may be no need of salted flesh. For after the raw flesh hath been kept so long in vessels stopped with screws, as experience shews there is no danger of its corruption; then it is to be taken out, and being perfectly boiled is again to be included in the same receivers, and so without doubt it may be kept for a long time without salt. The chief art to preserve flesh without salt consists herein, that all air be excluded from it, and that there be a great compression in the receiver (*a*).

(*a*) The reason why spirit of wine preserves flesh, and other things immersed in it, from corruption may be, that the spirit sucks up and consumes the air lodged in the pores of the flesh. For it has been found that spirit of wine will imbibe a bubble of air as large as your thumb in about two hours, which is more than water will do in a much longer time, though it be first well purged of air by boiling it.

ART. XVIII. *Contains experiments concerning elixation and distillation in vacuo.*

ART. XIX. *Contains some experiments concerning elixation in vessels stopped with screws.*

By which it appears, that even harts-horn and the bones of fishes and four-footed creatures may be softened and converted into good nourishment.



A N

A P P E N D I X.

N U M B. I.

The reason of the rising and falling of the mercury in the barometer, upon change of weather, by Dr. Halley (a).

TO account for the different heights of the mercury at several times, it will not be unnecessary to enumerate some of the principal observations made upon the barometer.

1. The first is, that in calm weather, when the air is inclined to rain, the mercury is commonly low.

2. That in serene, good, settled weather the mercury is generally high.

3. That upon very great winds, though they be not accompanied with rain, the mercury sinks lowest of all, with relation to the point of the compass the wind blows upon.

4. That *cæteribus paribus*, the greatest heights of the mercury are found upon easterly and north-easterly winds.

5. That in calm frosty weather the mercury generally stands high.

6. That after very great storms of wind, when the quicksilver has been low, it generally rises again very fast.

7. That the more northerly places have greater alterations of the baroscope than the more southerly.

8. That within the tropicks and near them, those accounts we have had from others, and my own observations at St. Helena, make very little or no vari-

(a) Reprinted from *Lowthorp's Abridg. of the Philosophical Transactions*, vol. II. pag. 20.

ation of the height of the mercury in all weathers.

Hence I conceive, that the principal cause of the rise and fall of the mercury, is from the variable winds which are found in the temperate zones, and whose great inconstancy here in England is most notorious.

A second cause is the uncertain exhalation and precipitation of the vapours lodging in the air, whereby it comes to be at one time much more crouded than at another, and consequently heavier; but this latter in a great measure depends upon the former. Now from these principles I shall endeavour to explicate the several phænomena of the barometer, taking them in the same order I laid them down.

1. The mercury's being low inclines it to rain, because the air being light, the vapours are no longer supported thereby, being become specifically heavier than the medium wherein they floated; so that they descend towards the earth, and in their fall, meeting with other aqueous particles, they incorporate together and form little drops of rain. But the mercury's being at one time lower than at another, is the effect of two contrary winds blowing from the place where the barometer stands; whereby the air of that place is carried both ways from it, and consequently the incumbent cylinder of air is diminished, and accordingly the mercury sinks. As for instance, if in the German ocean it should blow a gale of westerly wind, and at the same time an easterly wind in the Irish sea, or if in France it should blow a northerly wind, and in Scotland a southerly, it must be granted me that, that part of the atmosphere impendent over England would thereby be exhausted and attenuated, and the mercury would subside, and the vapours which before floated in those parts of the air of equal gravity with themselves, would sink to the earth.

2. The

2. The greater height of the barometer is occasioned by two contrary winds blowing towards the place of observation, whereby the air of other places is brought thither and accumulated ; so that the incumbent cylinder of air being increased both in height and weight, the mercury pressed thereby must needs rise and stand high, as long as the winds continue so to blow ; and then the air being specifically heavier, the vapours are better kept suspended, so that they have no inclination to precipitate and fall down in drops ; which is the reason of the serene good weather, which attends the greater heights of the mercury.

3. The mercury sinks the lowest of all by the very rapid motion of air in storms of wind. For the tract or region of the earth's surface, wherein these winds rage, not extending all round the globe, that stagnant air which is left behind, as likewise that on the sides, cannot come in so fast as to supply the evacuation made by so swift a current ; so that the air must necessarily be attenuated when and where the said winds continue to blow, and that more or less according to their violence ; add to which, that the horizontal motion of the air being so quick as it is, may in all probability take off some part of the perpendicular pressure thereof : and the great agitation of its particles is the reason why the vapours are dissipated, and do not condense into drops so as to form rain, otherwise the natural consequence of the air's rarefaction.

4. The mercury stands the highest upon an easterly or north-easterly wind, because in the great Atlantick ocean, on this side the 35th degree of north latitude, the westerly and south-westerly winds blow almost always Trade, so that whenever here the wind comes up at east and north-east, it is sure to be checked by a contrary gale as soon as it reaches the o-

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cean ;

cean; wherefore, according to what is made out in our second remark, the air must needs be heaped over this island, and consequently the mercury must stand high, as often as these winds blow. This holds true in this country, but is not a general rule for others where the winds are under different circumstances; and I have sometimes seen the mercury here as low as 29 inches upon an easterly wind, but then it blew exceeding hard, and so comes to be accounted for by what was observed upon the third remark.

5. In calm frosty weather the mercury generally stands high, because (as I conceive) it seldom freezes but when the winds come out of the northern and north-eastern quarters, or at least unless those winds blow at no great distance off; for the northern parts of Germany, Denmark, Sweden, Norway, and all that tract from whence north-eastern winds come, are subject to almost continual frost all the winter; and thereby the lower air is very much condensed, and in that state is brought hitherwards by those winds, and being accumulated by the opposition of the westerly wind blowing in the ocean, the mercury must needs be prest to a more than ordinary height; and as a concurring cause, the shrinking of the lower parts of the air into lesser room by cold, must needs cause a descent of the upper parts of the atmosphere to reduce the cavity made by this contraction to an *æquilibrium*.

6. After great storms of wind, when the mercury has been very low, it generally rises again very fast. I once observed it to rise $1\frac{1}{2}$ inch in less than 6 hours after a long continued storm of south-west wind. The reason is, because the air being very much rarefied, by the great evacuations which such continued storms make thereof, the neighbouring air runs in the more swiftly to bring it to an *æquilibrium*; as

we see water runs the faster for having a great declivity.

7. The variations are greater in the more northerly places, as at Stockholm greater than at Paris (compared by Mr. *Pascall*) (*a*) because the more northerly parts have usually greater storms of wind than the more southerly, whereby the mercury should sink lower in that extream; and then the northerly winds bringing the condensed and ponderous air from the neighbourhood of the pole, and that again being checked by a southerly wind at no great distance, and so heaped, must of necessity make the mercury in such case stand higher in the other extream.

8. Lastly, this remark, that there is little or no variation near the equinoctial, as at Barbadoes and St. Helena, does above all others confirm the hypothesis of the variable winds being the cause of these variations of the height of the mercury; for in the places above named there is always an easy gale of wind blowing nearly upon the same point, viz. E.N.E. at Barbadoes, and E.S.E. at St. Helena, so that there being no contrary currents of the air to exhaust or accumulate it, the atmosphere continues much in the same state: however upon hurricanes (the most violent of storms) the mercury has been observed very low, but this is but once in two or three years, and it soon recovers its settled state of about $29\frac{1}{2}$ inches.

The principal objection against this doctrine is, that I suppose the air sometimes to move from those parts where it is already evacuated below the *æquilibrium*, and sometimes again towards those parts where it is condensed and crouded above the mean state, which may be thought contradictory to the laws of statics, and the rules of the *æquilibrium* of

(*a*) *Equilibre des liqueurs.*

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fluids. But those that shall consider how when once an impetus is given to a fluid body, it is capable of mounting above its level, and checking others that have a contrary tendency to descend by their own gravity, will no longer regard this as a material obstacle; but will rather conclude, that the great analogy there is between the rising and falling of the water upon the flux and reflux of the sea, and this of accumulating and extenuating the air, is a great argument for the truth of this hypothesis. For as the sea over against the coast of Essex, rises and swells by the meeting of the two contrary tides of flood, whereof the one comes from the S.W. along the channel of England, and the other from the north, and on the contrary sinks below its level upon the retreat of the water both ways, in the tide of ebb; so it is very probable, that the air may ebb and flow after the same manner; but by reason of the diversity of causes whereby the air may be set in moving, the times of these fluxes and refluxes thereof are purely casual, and not reducible to any rule, as are the motions of the sea, depending wholly upon the regular course of the moon.

NUMB. II.

A scale of degrees of heat by Sir ISAAC
NEWTON (*a*).

The signs and descriptions of heats.

Equal parts of heat. 0.		T HE heat of air in winter, when wa- ter begins to freeze. This heat may be exactly determined by placing a thermometer in compressed snow when it begins to thaw.
0, 1, 2		The heats of the air in winter.
2, 3, 4		The heats of the air in spring and autumn.
4, 5, 6		The heats of the air in summer.
6		The heat of the air at noon about the month of July.
12	I	The greatest heat which a thermo- meter can acquire in contact with a hu- man body: the heat of a bird hatching her eggs is much the same.
$14\frac{3}{4}$	$I\frac{1}{4}$	Almost the greatest heat of a bath that a person can bear while his hand is immersed and constantly agitated for some time. The heat of the blood just let out of the body is almost the same.
17	$I\frac{1}{2}$	The greatest heat of a bath that a person can bear, while his hand is im- mersed and kept constantly at rest for some time.
$20\frac{2}{4}$	$I\frac{3}{4}$	The heat of a bath in which floating wax, after it has been melted, begins by

(*a*) Translated from the original in the Philosophical Trans-
actions, N^o 270.

		cooling to lose its fluidity and transparency.
24	2	The heat of a bath, by which floating wax is so heated as to melt, and continue in fusion without ebullition.
$28\frac{6}{11}$	$2\frac{1}{4}$	A middle degree of heat between that wherewith wax melts and water boils.
34	$2\frac{1}{2}$	The heat with which water boils vehemently, and a mixture of 2 parts of lead, 3 of tin and 5 of bismuth grows stiff by cooling. Water begins to boil with a heat of 33 parts, and in boiling scarce ever exceeds a heat of $34\frac{1}{2}$. Drops of hot water falling upon hot iron, cease to bubble, when the iron has 35 or 36 parts of heat, and of cold water, when the iron has 37 parts.
$40\frac{4}{11}$	$2\frac{3}{4}$	The least heat with which a mixture of 1 part of lead, 4 of tin and 5 of bismuth, will melt and continue in fusion.
48	3	The least heat with which a mixture of equal parts of tin and bismuth melts. This mixture by cooling grows stiff with 47 parts of heat.
57	$3\frac{1}{4}$	The heat with which a mixture of 2 parts of tin and 1 part of bismuth melts; as also a mixture of 3 parts of tin and 2 of lead: but a mixture of 5 parts of tin and 2 parts of bismuth, grows stiff by cooling in this heat; and so does a mixture of equal parts of tin and bismuth.
68	$3\frac{1}{2}$	The least heat with which a mixture of 1 part of bismuth and 8 parts of tin melts,

81

 $3\frac{3}{4}$

melts. Tin by itself melts with a heat of 72 parts, and grows stiff by cooling in a heat of 70 parts.

The heat with which bismuth melts, as also a mixture of 4 parts of lead and 1 part of tin. But a mixture of 5 parts of lead and 1 part of tin, after it has been melted, grows stiff by cooling in this heat.

96

4

The least heat with which lead melts. It grows hotter and melts with a heat of 96 or 97 parts, and grows stiff by cooling in a heat of 95 parts.

114

 $4\frac{1}{4}$

The heat with which burning bodies by cooling cease to be visible in a dark night, and on the contrary by heating begin to shine in the same degree of darkness, but with so faint a light as is scarce sensible. With this heat a mixture of equal parts of tin and regulus martis, and also a mixture of 7 parts of bismuth and 4 parts of that regulus, grows stiff in cooling.

136

 $4\frac{1}{2}$

The heat with which burning bodies shine in a dark night but not at all in the twilight. With this heat a mixture of two parts of regulus martis and 1 part of bismuth, and also a mixture of 5 parts of regulus martis and 1 part of tin, grow stiff by cooling. Regulus alone grows stiff with a heat of 146 parts.

161

 $4\frac{3}{4}$

The heat wherewith bodies burning in the twilight, just before sun-rise or after sun-set, shine manifestly, but not at all or but very obscurely in broad daylight.

192

5

The heat of burning coals in a small fire made of bituminous pit-coal, and not blown with the bellows. Iron heated as hot as possible in this fire, has the same heat as the fire itself. But the heat of a small fire made of wood, is a little greater, having 200 or 210 parts; and the heat of a large fire is still greater, especially if blown with bellows.

In the first column of this table we have degrees of heats in arithmetical progression, beginning from the heat with which water just begins to freeze, as the lowest degree of heat, or as a limit common to heat and cold, and considering the external heat of a human body as consisting of 12 equal parts.

In the second column we have degrees of heats in geometrical progression; the first degree (12) is the external heat of a human body adjusted by our senses, the second (24) is double the first, the third (48) is double the second, the fourth (96) is double the third and the fifth (192) double the fourth (*b*).

By this table it appears, that the heat (34) of boiling water, is almost three times greater than the heat (12) of a human body, and the heat (72) of melting tin six times greater, and the heat (96) of

(*b*) I understand the Author's sense of this paragraph as follows. "In the second column we have" a scale of indices or exponents of "degrees of heat in geometrical progression." The numbers $1, 1\frac{1}{4}, 1\frac{1}{2}, 1\frac{3}{4}, 2, \&c.$ in the second column, being in arithmetical progression, are a scale of logarithms, or measures of the ratios of the heats expressed by the corresponding numbers 12, $14\frac{3}{4}, 17, 20\frac{1}{2}, 24, \&c.$ in the first column; which being in geometrical progression, may be soon found by taking $\frac{1}{4}, \frac{1}{2}, \frac{3}{4}$ of the logarithm of 2, or of the ratio of 1 to 2, and multiplying the corresponding absolute numbers, in the table of logarithms, by 12. Then by doubling the 4 last terms of the geometrical progression so found, you get the 4 next, and by doubling these you have the 4 next, and so on to the end of the scale.

melting

melting lead eight times greater, and the heat (146) of melting regulus about twelve times greater, and the heat (200) of a common fire about sixteen or seventeen times greater than the heat of a human body.

The table was constructed by the help of a thermometer and red-hot iron. By the thermometer I found the measures of all the heats as far as that which melts tin, and the measures of all the rest by red-hot iron. For the heat which the iron communicates to contiguous cold bodies in a given time, that is, the heat which it loses in a given time, is as the whole heat of the iron. Consequently if the time of its cooling be divided into any equal parts, the corresponding heats [both lost and retained] will decrease in a geometrical progression, and therefore may easily be found by a table of logarithms (*c*).

First then by a thermometer made with linseed-oil, I found when the thermometer was placed in melting snow, if the oil took up 10000 equal parts of space, that the same oil, being afterwards rarefied and dilated by the first degree of heat, or that of a human body, took up 10256 such parts, and by the heat of water just beginning to boil 10705 parts, and by the heat of water boiling vehemently 10725 parts, and by the heat of melted tin, when by cooling it began to stiffen to the consistence of an amalgama 11516 parts, and when quite stiff 11496 parts.

Therefore the oil was rarefied and dilated in the ratio of 40 to 39 by the heat of a human body, in the ratio of 15 to 14 by the heat of boiling water, in the ratio of 15 to 13 by the heat of melted tin beginning to stiffen and coagulate by cooling, and in the ratio of 23 to 20 by the heat of tin just grown quite stiff.

(*c*) See pag. 101. paragraph. 1.

The

The rarefaction of air by an equal heat was ten times greater than that of the oil, and the rarefaction of the oil about fifteen (*d*) times greater than that of spirit of wine.

By these experiments, taking the heats of the oil to be proportional to its rarefactions (*e*), and for the external heat of a human body writing 12 parts, the heat of water just beginning to boil, comes out 33 such parts, and of water boiling vehemently 34 parts, and of tin either beginning to melt or to stiffen into an amalgama 72 parts, and of tin just become quite stiff and hard 70 parts.

Having found these heats, in order to determine the rest, I heated a piece of iron of a sufficient thickness, till it became red-hot, and taking it from the fire with the tongs likewise red-hot, I immediately put it in a cool place where the wind blew constantly, and upon it I laid particles of diverse metals and other fusible bodies, and noted the several instants of time when by cooling they lost their fluidity and began to coagulate, and lastly when the heat of the iron became equal to the external heat of a human body.

(*d*) The original runs thus, *Rarefactio aeris æquali calore fuit decuplo major quam rarefactio olei, & rarefactio olei quasi quindecim vicibus major quam rarefactio spiritus vini*, in which I conceive there is some mistake. For Dr. Halley, by an experiment described in the Philos. Transact. N^o 197. found that when spirit of wine began to boil (after which it has no regular expansion) it had increased itself a 12th part of its bulk when cold in winter time; and by my own trial I have found, that with the heat of spirit of wine beginning to boil, the linseed oil in my thermometer had increased itself about 53 thousandth parts of its bulk when placed in thawing snow, or to make a juster comparison, about $\frac{1}{10000}$ parts of its bulk when cold in winter time. Therefore the increment of the spirit, is to that of the oil, produced by equal heats, as $\frac{1}{12}$ to $\frac{1}{10000}$ or 501 to 306 or about 5 to 3.

(*e*) That is, to the increments of its bulk, as appears by the numbers in the two last paragraphs but one.

Then

Then upon this principle, that the excesses of the heats of the iron and coagulating particles, above the heat of the atmosphere, found by my thermometer, were in a geometrical progression, when the times were taken in an arithmetical progression, I determined all those heats.

I placed the iron not in a stagnating air, but in a wind blowing uniformly, that the air heated by the iron might immediately be driven away by the wind, and that cool air might continually succeed it with an uniform motion. For by this means equal parts of the atmosphere were heated in equal times, with degrees of heat proportionable to the heats of the iron.

Now the heats so determined were to one another in the same ratio's as the heats found by my thermometer, and therefore the principle I assumed, that the heats of the oil were proportionable to its rarefactions, is a true principle (*f*). So far Sir *Isaac Newton*.

Hence we learn the construction of a thermometer, which being once adjusted by experiment to any one degree of heat in the Author's scale, shall determine the rest artificially, and also the proportions of any other heats to those in the scale.

For this purpose, since a tube seldom happens to be perfectly cylindrical, it must be distinguished into parts equal in capacity, if not in length, as follows. First weigh the empty tube, then having filled its ball and about a ninth or tenth part of the tube with quicksilver, weigh it again and deduct the former weight from the latter; the difference is the weight of the inclosed quicksilver, which gives the weight of one hundredth part of it.

(*f*) This property of the rarefaction of linseed-oil was afterwards confirmed by an experiment made by Dr. *Brook Taylor*, and described in the *Philosoph. Transact.* N^o 376.

Mark the tube with a file at the surface of the inclosed quicksilver, and with an hundredth part of its weight, weigh out 8 or 9 parcels of the like quicksilver, and pour them one after another upon the inclosed quicksilver, marking the tube successively at the surface of each parcel.

Then with your compasses compare the intervals of the marks, and if they be equal to one another, divide each of them into ten equal parts, otherwise make the parts increase or decrease as the intervals do.

Thus the capacity of the tube will be distinguished into thousandth parts of that of the ball and contiguous part of the tube reaching up to the first mark.

Then put the tube into a frame, and by the side of it place a scale of thousandth parts exactly corresponding with the opposite marks upon the tube; and writing 1000 over against the first mark, number the rest in their order, as in FIG. 41.

On the opposite side of the tube over against the numbers 1000, 1012.8, 1025.6, 1038.4, 1051.2, 1064, 1076.8, &c. in arithmetical progression, write 0, 6, 12, 18, 24, 30, 36, &c. also in arithmetical progression, where putting marks, divide their several intervals into six equal parts if the intervals be equal, otherwise into parts proportionable to the intervals. And along the side of this scale, at the proper divisions answering to the numbers in the first column of the Author's scale, write the names of the several bodies whose degrees of heat are expressed by those numbers.

The scale for the given tube being thus constructed, what remains to be done is only to pour in linseed-oil, and adjust it to such a quantity, that, when the thermometer is placed in the heat of any one body marked upon the scale, and has acquired it very slowly and uniformly in every part, the surface of
the

the oil may rest exactly at the mark belonging to that heat ; that is, at 0 if the ball be placed in compressed snow just thawing, or at 34, if in water just beginning to boil ; and so for any other.

Artificers generally fill their thermometers with a glass-funnel, whose pipe is drawn out, like a capillary tube, to a length and slenderness sufficient to enter the tube of the thermometer and reach down to its ball. And if they happen to pour in too much liquor, they insert an empty capillary tube, which will attract the liquor by little and little, till a due quantity be left behind.

By a linseed-oil thermometer well graduated and adjusted as above, many more may soon be made with oil or any other expanding fluid, without the trouble of graduating their tubes by equal quantities of quicksilver. For having filled the balls and a convenient part of the tubes, with the fluids proposed, place them all together in a skillet of cold water, and while it is warming as gently as possible, when the oil in the standard thermometer shall arrive successively at the several divisions of its scale, at the same instants of time mark the new tubes at the several heights of their fluids ; and form a scale for every tube that shall correspond to those marks. Then while the liquors subside by cooling gently, examine whether they correspond at the respective marks.

It is easy to understand how, by the help of quicksilver as above, the scale of thousandth parts in the standard thermometer, may be continued below the mark 1000 ; from which the scales of the new tubes may be continued downwards, by placing them all together in some freezing mixture, provided the liquors in the tubes be incapable of freezing. These parts of the scales are necessary for registering degrees of cold.

A thermometer that shall vary very sensible by every small variations of heat and cold, as those of the atmosphere, must have a large ball in proportion to the bore of the tube; and that the heat or cold may sooner diffuse itself, even to the innermost parts of the included liquor, the ball should not be spherical but oblong and flatted like a French flask. Thermometers intended for different uses should have tubes of different sizes and lengths, for comprehending a greater or smaller number of degrees of heat, suitable to the intended uses. And it may not be improper to place a spirit of wine thermometer in the same frame with that of linseed-oil, by which the larger intervals in the new scale may be determined; and these may be subdivided into smaller, answering to equal capacities of the tube, as above.

Being very much pleased with the ingenuity of the Author's method of measuring heats, and his care and judgment in selecting so great a variety of particulars as should compose a scale so remarkably regular, for further satisfaction in the accuracy of his measures, I formerly made an oil thermometer in the manner I have been describing; and having placed it in several heats mentioned by the Author, beginning from that of water just freezing and increasing to that of its boiling vehemently, I found they raised my oil exactly enough to the marks determined upon my scale; and judging Sir *Isaac Newton* to be the Author of this admirable paper, as well by the style and manner of it, as by its analogy to some passages in his writings, I was afterwards confirmed in my opinion by talking with him about it, and mentioning the agreement of the scale with my own experiments. Which I now mention as an experimental proof of the proposition I began with, and as an inducement to those that use thermometers in philosophical enquiries, to construct them in the manner

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ner here described, at least till a better be invented; in order to render their experiments intelligible and useful to the world. For it cannot but appear to a thinking person, even from the theory alone, that all thermometers of this construction, notwithstanding any difference in the shapes and capacities of the tubes, must needs agree together artificially, to the same degree of exactness, as bodies of the same name and sort, and in the same circumstances, can agree together in the quantity of their sensible qualities. And this I think is all the information a thermometer can give us, and what I presume has been hitherto wanting, in those at least that I ever met with.

NUMB. III.

An account of some experiments shown before the Royal Society; with an enquiry into the cause of the ascent and suspension of water in capillary tubes, by Dr. Jurin (a).

SOME days ago a method was proposed to me by an ingenious friend, for making a perpetual motion, which seemed so plausible, and indeed so easily demonstrable from an observation of the late Mr. Hawksbee, said to be grounded upon experiment, that though I am far from having any opinion of attempts of this nature, yet I confess I could not see why it should not succeed. Upon trial indeed the fallacy discovered itself. But as searchers after things impossible in themselves are frequently observed to produce other discoveries, unexpected

(a) Reprinted from the Philosoph. Transact. N^o 355.

by the inventor ; so this proposal has given occasion, not only to rectify some mistakes into which we had been led by that ingenious and useful member of the Royal Society above-named, but likewise to detect the real principle by which water is raised and suspended in capillary tubes, above the level.

I. My friend's proposal was as follows. In FIG. 42 let abc be a capillary siphon, composed of two legs ab , bc , unequal both in length and diameter; whose longer and narrower leg ab having its orifice a immersed in water, the water will rise above the level, till it fills the whole tube ab , and will then continue suspended. If the wider and shorter leg bc , be in like manner immersed, the water will only rise to some height as fc , less than the entire height of the tube bc .

This siphon being filled with water, and the orifice a sunk below the surface of the water de , my friend reasons thus.

Since the two columns of water ab and fc , by the supposition, will be suspended by some power acting within the tubes they are contained in, they cannot determine the water to move one way or the other. But the column bf , having nothing to support it, must descend, and cause the water to run out at c . Then the pressure of the atmosphere driving the water upward through the orifice a , to supply the vacuity, which would otherwise be left in the upper part of the tube bc , this must necessarily produce a perpetual motion, since the water runs into the same vessel, out of which it rises. But the fallacy of this reasoning appears upon making the experiment.

EXP. 1. For the water, instead of running out at the orifice c , rises upward towards f , and running all out of the leg bc , remains suspended in the other leg to the height ab .

EXP. 2. The same thing succeeds upon taking the siphon out of the water, into which its lower orifice

orifice a had been immerst, the water then falling in drops out of the orifice a , and standing at last at the height ab . But in making these two experiments it is necessary that ag the difference of the legs exceed fc , otherwise the water will not run either way.

EXP. 3. Upon inverting the siphon full of water, it continues without motion either way.

The reason of all which will plainly appear, when we come to discover the principle by which the water is suspended in capillary tubes.

II. Mr. *Hauksbee*'s observation is as follows. In FIG. 43 let $abfc$ be a capillary siphon, into which the water will rise above the level to the height cf , and let ba be the depth of the orifice of its longer leg below the surface of the water de . Then the siphon being filled with water, if ba be not greater than cf , the water will not run out at a , but will remain suspended.

This seems indeed very plausible at first sight. For since the column of water fc will be suspended by some power within the tube, why should not the column ba , being equal to, or less than the former, continue suspended by the same power?

EXP. 4. In fact, if the orifice c be lifted up out of the water de , the water in the tube will continue suspended, unless ba exceed fc .

EXP. 5. But when c is never so little immerst in the water, immediately the water in the tube runs out in drops at the orifice a , though the length ab be considerably less than the height cf .

Mr. *Hauksbee* in his Book of Experiments has advanced another observation, namely, that the shorter leg of a capillary siphon, as $abfc$, must be immerst in the water to the depth fc , which is equal to the height of the column, that would be suspended in it, before the water will run out at the longer leg.

EXP. 6. From what mistake this has proceeded, I

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cannot

cannot imagine; for the water runs out at the longer leg, as soon as the orifice of the shorter leg comes to touch the surface of the stagnant water, without being at all immerst therein.

III. I proceed now to enquire into the cause of the ascent and suspension of water in capillary tubes.

That this phænomenon is no way owing to the pressure of the atmosphere, has been, I think sufficiently proved by Mr. *Hauksbee's* experiments.

And that the cause assigned by the same person, namely, the attraction of the concave surface, in which the suspended liquor is contained, is likewise insufficient for producing this effect, I thus demonstrate.

Since in every capillary tube the height, to which the water will spontaneously ascend, is reciprocally as the diameter of the tube, it follows, that the surface containing the suspended water in every tube is always a given quantity: but the column of water suspended, is as the diameter of the tube. Therefore, if the attraction of the containing surface be the cause of the water's suspension; it will follow, that equal causes produce unequal effects, which is absurd.

To this it may perhaps be objected, that in two tubes of unequal diameters, the circumstances are different, and therefore the two causes, though they be equal in themselves, may produce effects that are unequal. For the lesser tube has not only a greater curvature, but those parts of the water, which lie in the middle of the tube, are nearer to the attracting surface, than in the wider. But from this, if any thing follows, it must be, that the narrower tube will suspend the greater quantity of water, which is contrary to experiment. For the columns suspended are as the diameters of the tubes.

But as experiments are generally more satisfactory in things of this nature, than mathematical reasonings,

ings, it may not be amiss to make use of the following, which appear to me to contain an *experimentum crucis*.

In FIG. 44, the tube *cd* is composed of two parts, in the wider of which the water will rise spontaneously to the height *bf*, but the narrower part, if it were of a sufficient length, would raise the water to a height equal to *cd*.

EXP. 7. This tube being filled with water, and the wider end *c* immerst in the stagnant water *ab*, the whole continues suspended.

EXP. 8. The narrower end being immerst, as in FIG. 45, the water immediately subsides, and stands at last at the height *dg* equal to *bf*.

From which it is manifest, that the suspension of the water in the former of these experiments is not owing to the attraction of the containing surface; since, if that were true, this surface being the same, when the tube is inverted, would suspend the water at the same height.

IV. Having shewn the insufficiency of this hypothesis, I come now to the real cause of that phænomenon, which is the attraction of the periphery, or rather of the small annular portion of the inside of the tube, to which the upper surface of the water is contiguous and coheres.

For this is the only part of the tube, from which the water must recede upon its subsiding, and consequently the only one, which by the force of its cohesion or attraction, opposes the descent of the water.

This likewise is a cause proportional to the effect, which it produces; since that periphery, and the column suspended, are both in the same proportion as the diameter of the tube.

Though from either of these particulars it were easy to draw a just demonstration, yet to put the

matter out of all doubt, it may be proper to confirm this assertion, as we have done the former, by actual experiment.

Let therefore, in FIG. 46, *edc* be a tube, like that made use of in the 7th and 8th experiments, except that the narrower part is of a greater length; and let *af* and *bg* be the heights, to which the water would spontaneously rise in the two tubes *ed* and *dc*.

EXP. 9. If this tube have its wider orifice *c* immersed into the water *ab*, and be filled to any height less than the length of the wider part, the water will immediately subside to a level with the point *g*; but if the surface of the contained water enter never so little within the smaller tube *ed*, the whole column *dc* will be suspended, provided the length of that column do not exceed the height *af*.

In this experiment it is plain, that there is nothing to sustain the water at so great a height, except the contact of the periphery of the lesser tube, to which the upper surface of the water is contiguous. For the tube *dc*, by the supposition, is not able to support the water at a greater height than *bg*.

EXP. 10. When the same tube is inverted, as in FIG. 47, and the water is raised into the lower extremity of the wider tube *cd*, it immediately sinks, if the length of the suspended column *db* be greater than *gb*; whereas in the tube *de* it would be suspended to the height *af*. From which it manifestly appears, that the suspension of the column *db* does not depend upon the attraction of the tube *de*, but upon the periphery of the wider tube, with which its upper surface is in contact.

V. For the sake of those, who are pleased with seeing the same thing succeed in different manners, we subjoin the two following experiments, which are in substance the same with the 9th and 10th.

In FIG. 48, *abc* is a siphon, in whose narrower
and

and shorter leg ab , if it were of a sufficient length, might be suspended a column of water of the height ef ; but the longer and wider leg bc will suspend no more than a column of the length gb .

EXP. 11. This siphon being filled with water, and held in the same position, as in the figure, the water will not run out at c the orifice of the longer leg, unless dc the difference of the legs ab and bc , exceed the length ef .

EXP. 12. If the narrower leg bc be longer than ab , as in FIG. 49, the water will run out at c , if dc the difference of the legs exceed ef ; otherwise it will remain suspended.

In these two experiments it is plain, that the columns dc are suspended by the attraction of the peripheries at a , since their lengths are equal to ef , or to the length of the column, which by the supposition those peripheries are able to support; whereas the tubes bc will sustain columns, whose lengths are equal to gb .

VI. Though these experiments seem to be conclusive, yet it may not be improper to prevent an objection, which naturally presents itself, and which at first view may be thought sufficient to overturn our theory.

For since a periphery of the tube ed , in FIG. 46, is able to sustain no more than a column of the length af , contained in the same tube; how comes it to sustain a column of the same length in the wider tube dc , which is as much greater than the former, as the section of the wider tube exceeds that of the narrower?

Again, if a periphery of the wider tube dc , in FIG. 47, be able to sustain a column of water in the same tube, of the length bg ; why will it support no more than a column of the same length in the narrower tube ed ?

Which queries may likewise be made with regard to the 11th and 12th experiments.

The answer is easy, for the Moments of those two columns of water are precisely the same, as if the sustaining tubes *ed* and *cd*, were continued down to the surface of the stagnant water *ab*; since the velocities of the water, where those columns grow wider or narrower, are to the velocities at the attracting peripheries, reciprocally as the different sections of the columns.

EXP. 13. From which consideration arises this remarkable paradox, that a vessel being given of whatsoever form, as *abc*, in FIG. 50, and containing any assignable quantity of water, how great soever; that whole quantity of water may be suspended above the level, if the upper part of the vessel *c* be drawn out into a capillary tube of a sufficient fineness.

But whether this experiment will succeed, when the height of the vessel is greater than that, to which water will be raised by the pressure of the atmosphere, and how far it will be altered by a *vacuum*, I shall give an account in my next paper.

Having discovered the cause of the suspension of water in capillary tubes, it will not be difficult to account for the seemingly spontaneous ascent of it; for since the water that enters a capillary tube, as soon as its orifice is dipt therein, has its gravity taken off by the attraction of the periphery, with which its upper surface is in contact, it must necessarily rise higher, partly by the pressure of the stagnant water, and partly by the attraction of the periphery immediately above that, which is already contiguous to it.

NUMB. IV.

An account of some new experiments relating to the action of glass tubes upon water and quicksilver, by Dr. Jurin (a).

I. **I**N the foregoing discourse, presented to the Royal Society, I maintained that the suspension of water in a capillary tube was owing to the attraction of a small annular surface on the inside of the tube, which touched the upper part of the water. Among the several experiments made use of to prove this assertion, was that of a glass funnel of several inches diameter, having its small end drawn out into a very fine tube, which funnel being inverted and filled with water, the whole quantity of water therein contained was sustained above the level by the attraction of that narrow annulus of glass, with which the upper surface of the water was in contact.

Soon after that discourse was printed, came out a book published by a learned and ingenious member of this Society, in which that experiment was accounted for in the following manner.

FIG. 51. *If there be a funnel, as ABC, full of water, and whose wide end stands in a vessel of water as BC; and the top of the funnel A ends in a capillary tube open at A, the whole water will be sustained: the pillar Aa by the attraction of the circle of glass within the tube immediately above it; and all the rest of the pillars of water, as Ff, Dd, Ee, Gg, &c. in some measure by the attraction of the parts of the glass above them, as F, D, E, G: and that the small pillars or threads of water Dd and Ee, do not slide down to Ff and Gg, and so go*

(a) Reprinted from the Philosoph. Transact. N^o 363.

quite down, seems to be owing to their cohesion with the pillar Aa, which is sustained by the capillary tube A: for if you break off the said tube at DE, the whole water will presently sink down.

As this solution was very different from what I had before given, and the reputation of that gentleman, whose great knowledge in experimental philosophy is generally known, was sufficient to give weight to any of his opinions; I thought myself under an obligation to examine his account of the experiment, in order either to demonstrate its insufficiency, or to retract my own solution. Accordingly at the next meeting of the Society, I produced the following experiment.

In FIG. 52, the funnel *afgbc*, whose lower part *bcsf*, was cylindrical to a considerable height, and whose top was drawn out into a fine tube at *a*, being filled with water to the height *bf*, so that the surface of the water *fg*, did not reach to the arched part of the funnel; I touched the end *a* with a wetted finger, whereby a small quantity of water being insinuated into the capillary tube at *a*, the water contained in the funnel was suspended above the level of the water in the cistern *de*, as in the former experiment.

In this experiment it is manifest, that the little columns, into which we may suppose the cylinder of water, *fgbc*, to be divided, are no way sustained by the attraction of the arched part of the glass above them, since they have no contact with it. Nor is there any such middle pillar of water, which, by its contact with the tube at top, is both sustained itself, and helps to support the pillars about it. Upon the supposition of which two particulars, that gentleman's solution was founded.

This experiment may be thus accounted for. The cylinder of water *fgbc*, by its weight ballances a part of the pressure of the atmosphere, which is incumbent

bent on the water in the cistern, and endeavours to force that cylinder upwards. The rest of that pressure is ballanced by the spring of the air, *afg*, which is included between the cylinder of water *fgbc*, and the little column of water in the capillary *a*. But, as this air by its spring presses equally every way, it must ballance as much of the pressure of the atmosphere upon the little column of water at *a*, as it does of that upon the water in the cistern. The remainder of the pressure of the atmosphere upon the column of water at *a* is sustained by the force, with which that column adheres to the capillary tube, which therefore does exactly ballance the weight of the cylinder of water *fgbc*, and is the real, though not the immediate, cause of its suspension.

The experiment succeeds in the same manner, when a column of quicksilver is raised into the funnel, instead of the column of water *fgbc*, the top of the tube being touched with a wet finger as before. But then the height of the quicksilver in the funnel must be as much less than that of the water, as its specifick gravity is greater.

I proceed now to acquit myself of a promise I made in the discourse abovementioned, of examining whether the experiments therein contained would succeed in *vacuo*; and whether water could be suspended in a wide tube by means of a capillary at top, at a greater height, than what it can be raised to by the pressure of the atmosphere.

In order to this, I boiled some water, and afterwards purged it of its air, by means of the air-pump; which being done, those experiments all succeeded in the exhausted receiver, in the same manner as in the open air.

The 13th experiment in particular, was made with a tube of about 35 inches in length, and a quarter of an inch diameter, the top of it being drawn out
into

into a fine capillary. Which being filled with water purged of its air, as before mentioned, the whole quantity continued suspended in the exhausted receiver.

This plainly shews, that the success of that experiment does not depend upon the pressure of the air, since the small quantity of air left in the receiver was by no means capable of sustaining the water at so great a height, and consequently that the height, at which water may be suspended in this manner, is not limited by that pressure.

But here I must not omit taking notice of a considerable difficulty, which presents itself to those who attentively consider this experiment. In order to make which the better appear, it will be proper to observe, what happens, when a simple capillary tube is filled with water purged of air, and inclosed in the exhausted receiver.

In this case the whole column of water contained in the tube, *acb*, FIG. 53, is suspended by the attraction of the *annulus* at the top of the tube, *a* : and though that *annulus* does not immediately act upon any part of the water, except what is either contiguous to it, or so near as to be within the sphere of its attraction, which extends but to a very small distance ; yet it is impossible, that any other part of the water, as for instance that at *c*, should part from the water above it, and sink down ; because its descent is opposed by the attraction of the contiguous *annulus* at *c*. For this being equal to the upper *annulus* at *a*, is capable of sustaining a column of water of the length *ab*, and consequently is more than sufficient for supporting the column of water below it, *cb*. From which it is plain, that no part of the water contained in the tube can possibly descend, unless the upper part, assisted by the weight of the water

ter

ter below it, be sufficient to overcome the attraction of the *annulus* of glass at *a*.

But in such a compound tube, as that made use of in our experiment, *acb*, FIG. 54, the case is very different, and it does not easily appear, why in a *vacuum* any part of the water in the wider part of the tube, as for example at *c*, should not leave that which is above it and descend; since the *annulus* at *c* is by much too wide to sustain a column of water of so great a length as *cb*.

The best answer I can give to this difficulty is, that the cohesion between the water contained in the capillary and that below it, is sufficient to ballance the weight of the column suspended. But how far this cohesion may depend upon the pressure of a medium, subtle enough to penetrate the receiver, is worthy of consideration. For though such a medium will pervade the pores of the water, as well as those of the glass, yet it will act with its intire pressure upon all the solid particles, if I may so call them, of the surface of the water in the cistern; whereas so many of the solid particles of the water in the tube, which happen to lie directly under the solid particles of the water above them, will thereby be secured from this pressure; and consequently there will be a less pressure of this medium upon any surface of the water in the tube below the capillary, than upon an equal surface of the water in the cistern. So that the column of water suspended in the tube may be sustained by the difference between those two pressures. This explication seems to be favoured by the following experiments, which may all be accounted for in the same manner, though I shall anon mention another cause, which contributes to the success of the first and second.

The first I shall mention is the famous experiment of the suspension of mercury purged of air, to the height

height of 70 or 75 inches, in the Torricellian-tube, in the open air. To which we may add the sustaining of mercury, likewise purged of air, within the exhausted receiver, as related by the learned *Monf. Papin* in his *Continuation du Digesteur*. I forbear to mention the suspension of water purged of air in the *Vacuum*, which he describes in the same book; because there is little difference between that experiment and our own above-mentioned; the very top of the arched part of his tube, which top we may suppose as small as we please, supplying the place of the fine capillary at the top of our tube. But we must not omit the experiments made by the famous *Monf. Huygens*, and described by him in *Philosoph. Transact.* N° 86, of the cohering of polished plates, with a considerable force in the exhausted receiver; as likewise of the running of water and mercury, when purged of air, through a siphon of unequal legs in the *vacuum*: all which he accounts for from the same principle, and much in the same manner, as we have used for explaining the experiment above.

III. As to the existence of such a medium, I shall content myself to refer to what has been said by our illustrious President in the *Queries* at the latter end of the last edition of his *Opticks*. And as I have lately had the honour to entertain the Society with some experiments upon quicksilver, which were exactly the reverse of those made by *Dr. Taylor*, the late *Mr. Hawksbee* and myself, upon water; by which I am now enabled to throw this whole affair into a little system by itself, I shall lay it down in the following propositions, the proof of which is contained in the experiments annexed.

Prop. 1. *The particles of water attract one another.*

This, I think, is now universally acknowledged, and therefore needs no demonstration; the sphericity of the drops of rain, and the running of two
drops

drops of water into one another upon their contact, manifestly proving it.

Prop. 2. *The particles of quicksilver attract one another.*

This is likewise manifest from the spherical figure, into which a drop of mercury forms itself upon a table; and from two of them immediately running together, as soon as they come to touch.

Prop. 3. *Water is attracted by glass.*

This plainly appears from all the experiments, that we have shewn upon this subject.

Prop. 4. *Quicksilver is attracted by glass.*

Exp. 1. If a small globule of quicksilver be laid upon a clean paper, and be touched with a piece of clean glass; upon drawing the glass gently away, the quicksilver will adhere to it, and be drawn away with it. And if the glass be lifted up from the paper, the quicksilver will be taken up by it, in the same manner as a piece of iron is drawn up by the loadstone, and will stick to the glass by a plain surface of a considerable breadth, in proportion to the bulk of the drop, as manifestly appears by an ordinary microscope. Then if the glass be held a little obliquely, the drop of mercury will roll slowly upon its axis along the under side of the glass, till it comes to the end, where it will be suspended as before.

Exp. 2. If a pretty large drop of mercury be laid upon a paper, and two pieces of glass be made to touch it, one on each side; upon drawing the glasses gently from each other, the drop of mercury will adhere to them both, and will be visibly drawn out from a globular to an oval shape; the longer axis passing through the middle of those surfaces, in which the drop touches the glasses.

Prop. 5. *The particles of water are more strongly attracted by glass than by one another.*

This manifestly appears from the rising of water in small tubes above the level. For when the water
begins

begins to rise into a capillary tube, all the particles of water, which touch the small *annulus* at the bottom of the tube, must have quitted the contact of the other water, and have risen contrary to their gravity, to come into contact with the glass. After the same manner the other experiments of Dr. Taylor, Mr. Hawksbee and myself, upon this subject, are easily explicable. For upon a careful examination, it will be found in them all, that some parts of the water quit the contact of the other water, and join themselves to the glass.

Prop. 6. *The particles of quicksilver are more strongly attracted by one another, than by glass.*

Exp. 1. If a small tube as *ab*, FIG. 55, open at both ends, be dipt into a glass vessel filled with mercury, and be held close to the side of the vessel, that the rise of the mercury within it may appear; the mercury will partly enter into the tube, but will stand within it at some depth, as *ce*, below *cd* the surface of the quicksilver in the vessel, and this depth will always be reciprocally as the diameter of the tube.

In this experiment a column of quicksilver of the height *ce* endeavours to force the mercury higher into the tube; and as glass has been already proved to attract quicksilver, the attraction of the annular surface on the inside of the tube, which is contiguous to the upper part of the mercury, will likewise conspire to farther its ascent. What opposes the ascent of the quicksilver, is the power, by which that part of it, which endeavours to rise into the glass, is drawn back by the attraction of the other mercury, with which it is in contact laterally, and this does not only ballance the attraction of the glass, but likewise the weight of a column of mercury of the height *ce*, and consequently this attraction is considerably stronger than the attraction of the glass.

The

The cause therefore, that suspends the weight of the column of mercury ce , being the difference between the attraction of the annular surface of the tube at e , and that of an equal surface of the quicksilver in the cistern, from which the mercury, that endeavours to rise into the tube, must recede, in order to unite itself to such an annulus of the glass, will always be proportional to that annular surface, or to the diameter of the tube. And since the column sustained must be proportional to the cause that suspends it, that column must likewise be as the diameter of the tube. But the column suspended is as the square of the diameter of the tube, and the height ce conjointly; from which it follows, that the height ce must be as the diameter of the tube reciprocally, as it is found to be by experiment.

The experiment of the ascent of water above the level in a capillary tube, is just the reverse of this.

EXP. 2. Quicksilver being poured into the inverted siphon acb , FIG. 56, one of whose legs ac is narrower than the other cb ; the height ce , at which the mercury stands in the wider leg cb , is greater than the height cd , at which it stands in the narrower leg ca .

On the contrary, water stands higher in the narrower leg, than in the wider.

EXP. 3. In FIG. 57, $abcd$ represents a rectangular plane of glass, which makes one side of a wooden box. On the inside of this is another glass plane of the same size, which at the end ac is prest close to the former, and opens to a small angle at the opposite end bd . When mercury is poured into this box to any height as ce , it insinuates itself between the two glass planes, and rising to different heights between the glasses, where the opening is greater or less, it forms the common hyperbola cgf ; one of whose asymptotes ef is the line on which the surface
of

of the mercury in the box touches the inner glass; the other is the line *ac*, in which the planes are joined. This hyperbola being carefully examined by Mr. *Hauksbee* and myself, the rectangles *ebg*, wheresoever taken, proved always equal to one another, to as great an accuracy as could be expected, when the planes were opened to any considerable angle: but when the opening was very small, the inequalities of the planes, though the best I could procure, bearing a greater proportion than before to the distance between them, occasioned a sensible variation. Which, by the way, I take to be the reason, why the ordinates found the late Mr. *Hauksbee*, in examining the curve produced in a contrary situation, upon dipping two glass-planes so joined into spirit of wine, do not answer to those of the hyperbola.

EXP. 4. In FIG. 58, *ab* is a perpendicular section through two glass planes joined at *a*, and opened to a small angle at *b*; *c* represents a pretty large drop of mercury, the larger the better, which being made to descend as far as *c*, by holding the planes in an erect posture, with the end *a* downwards, retires from the contact of the planes to *d*, upon inclining the planes towards an horizontal situation; and the distance *cd* becomes greater or less, as the planes are more or less inclined towards the horizon.

A drop of any oily or watery liquor moves the contrary way, as has been shewn by the late Mr. *Hauksbee*.

EXP. 5. In FIG. 59, *ab* is a tube open at both ends, and a foot or two in length, whose lower part is drawn out into a fine capillary at *b*. This tube being filled with mercury, the whole column of quicksilver will be sustained in it, provided the capillary tube at *b* be sufficiently small. But if the mercury in the end *b* be suffered to touch any other mercury, it runs all out of the tube. If, without letting it touch
any

any other mercury, a small part of the end b be broken off, the mercury will run out, till it comes to some lesser height as bc , at which it will again stop, the height bc being nearly in a reciprocal proportion to the diameter of the small end of the tube.

The seventh experiment in the former paper is the reverse of this.

Exp. 6. Is the same in substance with the former, but made with a large glass-funnel ab , instead of a tube, FIG. 60.

The reverse of this in water is the thirteenth experiment in the former paper.

In all these experiments it is easily seen, that the effect is owing to the difference between the two attractions, by which mercury tends to glass and to its own body; they being always opposed to one another, so that a particular explication is no way necessary. But perhaps it may save some little trouble to the reader, to remove the following objection, which will readily occur to him.

In the experiments brought to demonstrate the fourth proposition, the globule of mercury adheres to the glass in a plane surface, which cannot be done without increasing the surface of the globule, and consequently removing some of its particles from the contact of one another. If therefore they tend more strongly to one another than to the glass, why do they not recede from the glass, and assume a figure perfectly spherical, that they may all have the greatest contact with each other?

To this we may answer, that the power by which mercury is attracted either by glass, or by other mercury, is proportional to the attracting surface; and therefore, though, *cæteris paribus*, the tendency of mercury to glass, is not so strong as its tendency to other mercury, yet in this case a much greater

number of mercurial particles coming into contact with the glass, than what recede from the contact of one to another, it is no wonder that the attraction of the glass prevails, and causes the globule to adhere to it. For the number of mercurial particles which lose their contact with the other mercury, is no more than what makes up the difference of surface, which arises from changing the figure of the drop; whereas the particles, which by this means come to adhere to the glass, are all those that constitute the plane surface, in which the globule touches it.

Which consideration ought likewise to be applied to the suspension of quicksilver in glass-tubes, either at extraordinary heights in the open air, or at lesser heights in a *vacuum*, as above-mentioned. For the top of the tube being spherical, or nearly so, it will be found, that the contact of the mercury with the extremity of the tube, is to the contact with other mercury, which would be gained by its leaving the top of the tube, and descending a very small space in a ratio infinitely great; and consequently that the contact of the mercury with the top of the tube is one cause of its suspension.

Corol. 1. From this proposition it appears, that in a barometer made with a narrow tube, the quicksilver will never stand at so great a height as in a wider. Which accounts for the phænomenon so often mentioned, in the yearly history of the Royal Academy of Sciences at Paris, by *Monf. De la Hire*; that in the barometer, which he constantly made use of for his annual observations, the quicksilver did not rise so high, as in another he kept by him, by about three lines and a half, which is near a third of an inch our measure: for he tells us, that the tube of his barometer is very small. So that there is no need to have recourse to any peculiarity, either in the quicksilver or the glass of which that tube was made; or

to

to an unperceived remnant of air left in the tube, from some of which causes that effect, and some others of the same kind were imagined to proceed.

Corol. 2. In a barometer made with a small tube, the mercury will rise and fall irregularly. For, as the height of the mercury depends partly upon the diameter of that part of the tube that touches the upper surface of the mercury, it is plain, that the unavoidable inequalities in the diameter of the tube will be more considerable, in respect to the whole diameter; and consequently will affect the height of the mercury more in a small tube than in a wider. And this I take to be the reason why it is so very difficult, not to say impossible, to make two barometers which shall exactly agree in the height of the quicksilver in all constitutions of the air, especially if the tubes be very narrow. This irregularity is still more considerable in the pendent barometer, in which the quicksilver moves through a large space, in order to make a small alteration in the length of the column suspended. The same consideration is easily extended to those levels, that depend upon the rising of mercury to the same height, in the opposite legs of a bent tube; an instrument of which kind has been lately offered. And as the effect is just contrary in levels made with water or spirit of wine, due regard ought to be had to this property in the construction of those instruments, by making the tubes sufficiently wide, in order to diminish the error as much as possible.

ERRATA.

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- 9, **N**OTE (b) sect. 2. insert Fig. 5.
 9, Note (c) insert Fig. 6.
 13, line 12, r. the plate.
 20, last line, r. within the legs, when the syphon is full, will
 &c.
 40, Note (a) line 3, for vial, r. bottle.
 41, line 4, r. than.
 42, Note (b) line 8, read, $A \rightarrow B$.
 44, line 24, read, tell.
 59, line 6, r. might be.
 60, line 3, r. into *England*.
 61, in the Table, r. Dry Maple ———o. 755.
 62, line 32, r. sulphureous.
 75, line 17, r. posteriori.
 76, line 22, for ascend, r. descend.
 83, line 17, r. Peripateticks.
 84, line 6, *dele* the.
 84, line 24, r. filled with.
 88, line 24, r. breath.
 109, line 10, for less, read legs.
 109, line 31, r. *æquilibrium*.
 113, line 14, *dele* the.
 113, line 21, r. up to 34.
 133, line 23, r. afforded.
 240, line 12, r. found by, *dele* in.

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